FÆCAL FERMENTATION

AS A

Cause of Disease:

TOGETHER WITH

THE GENERAL RULES OF TREATMENT TO BE OBSERVED.

BY

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FÆCAL FERMENTATION,

ETC.

PART I.

In the following paper, my object will be to direct renewed attention to a subject which had of late years been too much neglected, until Dr. Snow first recalled public attention to it. I allude to the fact that fæcal matters develope, under certain circumstances, a contagious poison, which becomes a powerful exciting cause of disease. I do not deny that those diseases to which I shall presently allude may be generated by the decomposition of other vegetable and animal matters, nor do I in any way assert that the fæcal poison is the *only one* which is in general operation; but I wish peculiarly to direct attention to this poison as a frequent cause in originating and continuing these diseases, and in afterwards rendering them fatally destructive. I also hope thereby to call the attention of our sanitary corps of officers to this source of disease, so that it may be tested by those facts, with which they will be sure to meet in the performance of their duties; and that both in regard to prophylaxis and treatment a broader and more general principle of action may be adopted. And because, in the same measures by which these fæcal poisons are withstood and counteracted, other vegetable and animal poisons are equally opposed and destroyed, so we may hope that the occurrence of many diseases, especially those of the epidemic and endemic varieties, may be prevented, or at least the *type* of such diseases may be rendered so much more benignant, that their treatment will become, under Providence, comparatively easy and successful. It is, therefore, with peculiar pleasure that I find the subject of malaria as a cause of disease in this country has been selected by the Council of this Society for the Fothergillian medal of 1858,

and I also trust that among the number of chemical students in the nation there may be found one or more able and courageous enough to grapple with the subject, and to overcome the natural repugnance to make investigations in such a department of science. If so, we may hope that the specific and chemical nature of some of these poisons may be found out.

To go over the whole subject, even in a cursory manner, in one paper, is to me at least impossible. I can only, therefore to-night, consider some points connected with the formation of fæcal matters, their subsequent decomposition in the production of particular poisons, the general laws under which these, in common with other poisons, act, and, finally, their effects when partaken of in substance by the lower animals, or by man, when this can be done. The effects of their ingestion or absorption in solution in water, and of the emanations therefrom in the production of disease, and the rules of prophylaxis and treatment, must form the subject of a second and a third paper. I must, therefore, claim your indulgence for the necessary imperfection of these remarks, and ask you to look upon this paper as only introductory to others, which, I trust, may prove of more interest, and in which I hope to develope the subject more fully and more practically.

One other point, and I have done with preliminary remarks. You must not expect much originality in a communication of this nature. The merit of the paper must be based on the number of facts collected from all sources. Nay more, the collection of evidence of this nature, because it is not mine, is more worthy of your credence. Otherwise, I might be open to the charge of moulding my facts to my theory; not that I believe you would think me capable of doing so willingly, but, even with the most mathematical construction of mind, it is often difficult to escape all bias in our convictions. The facts, moreover, may be all easily examined by each of you for yourselves, as, wherever I can do so, I will give you chapter and verse for all I state. My honest wish is to get at truth, not to pervert it.

CHEMICAL ANALYSES OF FÆCES.

It is a remarkable fact that the analyses of excrement, and especially the human variety, as made by different observers, not only do not agree, but throw after all but a very little light on many interesting points in its composi-

tion. The best and the most recent analysis that I am acquainted with is that of Dr. Marcet (*Philosophical Transactions*, vol. cxliv, p. 265). From his inquiries, it would appear that fæces consist of—excretine, a peculiar animal matter; margaric acid; colouring matter like that of blood; a light granular substance, which he regards as a combination of phosphate of potash and pure organic matter; an olive-coloured acid; excretoleic acid. Lastly, there is no evidence of butyric acid.

Excretine contains both sulphur and nitrogen. It is not found in carnivora. Their fæces yield a substance like it, but not identical with it. Moreover, these contain butyric acid in their excrements.

Among herbivora, horses, sheep, dogs fed on bread, wild boars, elephants, deer, and monkeys, neither butyric acid, excretine, nor cholesterine, are found. The crocodile, however, forms an exception in regard to the cholesterine.

From an analysis by Fleitmann, the salts of fæces are very numerous:—

Chloride of sodium	·58
Chloride of potassium	.07
Potash	12·44
Hydrate of potash (or equivalent weight of carbonate of potash)	10·05
Soda	0·75
Lime	21·36
Magnesia	10·67
Sesquioxide of iron	2·09
Phosphoric acid	30·98
Sulphuric acid	1·13
Silicic acid	1.44
Carbonic acid	1·05
Sand	7·39
	100·00

An ultimate analysis by Playfair gave the following results:—

Carbon	45·24
Hydrogen	6·88
Nitrogen (average)	4·
Oxygen	30·3
Ashes	13·58
	100·00

As a general rule, human excrement is richer in nitrogen than that of animals, in which the proportion of 2 per cent. only obtains; and even among men it varies with the quality of food, being among those who live on vegetables about the same as among herbivora, and greater among those whose food is essentially meat. About 7 per cent. at least of fæces consists of the undigested residue of food, such as pieces of muscle, lignine, chlorophyll, wax, etc. There is, also (varying with accidental circumstances), more or less of the secretions of the liver, pancreas, etc. Many of the changes, so brought about are chemical and mechanical changes occurring in the gut itself from the admixture of varied elements, left residuary during the transit through the intestines. Their occurrence is also best explained on this supposition. The production of fæces, viewed as such, is then simply one of limited oxidation or combustion. The fæces contain the unbruised substances of food, such as chlorophyll, wax, etc., which have suffered no change in the organism—the carbon, hydrogen, and nitrogen being small in quantity as compared with that in food. In fact, these indigestible matters may be compared to the smoke and soot produced when food is imperfectly burned in fireplace. (Playfair's *Liebig's Agricultural Chemistry*, fourth edition, p. 176.)

Still it cannot be denied that, in part at least, though to a very small degree in health, a portion of the fæcal matters are directly secreted from the mucous membrane of the intestines. This secretion, however, is at best only vicarious. Precisely as in amenorrhœa, hæmatemesis may supersede menstruation, or in jaundice the kidneys may eliminate the bile, so in disease or under extraordinary circumstances, a fæcal secretion may occur or be greatly exaggerated. Thus—

1st. Diarrhœa is often persistent, and, at the termination of some diseases, such as phthisis, will come on extensively, and yet after death there is but little fæcal matter found in the intestines. This matter must, then, have been secreted while life persisted.

2nd. In many cases, although the food taken is not greater than usual, the diarrhœal matters voided are unusually abundant.

3rd. Liebig has shown that if fibrine, albumen, or caseine, or gelatine, be treated with solid hydrate of potash, and the heat kept up until all the nitrogen is given off as ammonia, and the hydrogen begins to be given off, and the

residue be saturated with dilute sulphuric acid, and distilled, then a substance of fæcal colour, and of a horrible fæcal odour, which contains acetic and butyric acids, remains behind, which is, in fact, fæcal matter artificially prepared. "This odour varies with the nitrogenous substance employed, and thus we may have all the degrees of fæcal odour." (Carpenter.)

4th. Fæces differ in many respects in their odour from that evolved by animal substances during fermentation or putrefaction, this very odour disappearing on exposure, and the fæces themselves fermenting after such exposure. (Carpenter.)

5th. Besides the secretion of fluids, Magendie and Girardin have made an observation, which is confirmed by Frerichs, and which, at all events, proves the secretion of gas from the blood into the intestine; for if a loop of intestine in dogs, after being perfectly emptied of its contents, be tied at both ends, it is always found, after some time, filled with air. (*Lehmann's Chemistry*, Day's Translation, vol. ii, p. 132.) Copious gaseous secretions is often observed in fevers, when it gives rise to tympanitis.

The above view gives a ready explanation to many peculiarities observed in the production of fæcal matter during disease, and also to the great advantage which often follows copious purgation, which may be regarded as removing injurious matters from the blood precisely as other eliminants or evacuants; these matters having been probably often, at least in part, previously absorbed with the aliments in their progress through the intestinal canal.

FERMENTATIVE CHANGES.

The retention of fæcal matters in the body give rise to different diseases. Those in which fæcal matters, from their quantity, act, as it were, mechanically, and thus prove injurious, do not properly belong to the subject of this paper. It is to those which may be produced by the development of animal poisons formed in their substance through chemical action, either while in the body or after its expulsion out of it, that I wish especially to call attention.

These changes are essentially due to fermentation, in the full meaning of the term, including those of both the words putrefaction and eremacausis; that is, changes effected by exposure to water or atmospheric air at certain temperatures—eremacausis being the effect of a low combustion taking place in atmospheric air, without production

of odour, and putrefaction the same, with offensive odour. The changes which occur in fæces in the body in health, and out of the body under ordinary circumstances, are thus rather those of eremacausis; while those which occur in disease in the body, and under extraordinary circumstances out of it, are more properly those of putrefaction.

It is an admitted fact in the present day that many of the healthy changes in the body are the products of fermentation. All those different matters which constitute the food of animals are fermentescible substances. And all those varied substances which are capable of putrefaction, while in this state, have the property of acting as ferments. Thus, putrescent flesh, blood, bile, urine, the mucous membranes of the stomach, intestines, bladder, etc., the skin, etc., may act as ferments. To each of these particular ferments a peculiar kind of fermentation belongs, which may not be produced by another; and each results in the formation of a particular product. All that is necessary in this process is exposure to oxygen, which is usually effected through the agency of the atmosphere or of water. Indeed, many substances require the presence of the latter before they can absorb oxygen. This fermentation will continue till the molecular dynamisation set up has been again quieted by the production of a new substance, and, as a consequence, a state of equilibrium: and this equilibrium will persist so long as no new disturbing ferment is introduced, to set up another kind of fermentation. The fermenting principle will be reproduced just as yeast is from wort, whenever, first, there is a substance present with which the ferment was formed, and, secondly, whenever there is a compound capable of being decomposed by contact with the exciting ferment.

Now, there are some circumstances which, when in operation, favour putrefactive fermentation. These it may be as well to review, as a knowledge of them has a practical bearing to the subject under contemplation, more especially in regard to any prophylactic measures to be adopted. "It was long since shown by Barckmann and Hildenbrandt that oxygen is most conducive to putrefaction; but its power is greatly increased by the presence of nitrogen, which, however, appears to exert no other action than that of separating the molecules of oxygen. An analogous fact is presented by hypophosphoric acid, which is formed very readily when its base is in contact with a mixture of oxygen and azote, but not at all when submitted

to pure oxygen. Hildenbrandt found that meat putrefied completely in eleven days when exposed to this gas in a state of purity. Unmixed azote tends to retard rather than to favour putrefaction, hence this process is slow in privies. Hydrogen, carbonic acid, and nitrous acid are unfavourable, whereas electricity accelerates the process. Muscles subjected to electricity are, after a time, deprived of their salts, the oxides going to the negative pole, the acids to the positive. In the ordinary action upon animal matters, electricity most probably alters the composition of the proximate principles; thus, in milk, it developes acetic acid. Mathiei observes that portions of meat placed upon zinc plates remained fresh for a long time; the meat, being electro-negative, repelled the oxygen. Aqueous vapour is highly favourable. Gay Lussac found that he could retard putrefaction for a considerable period in a bell glass containing at the bottom of it chloride of calcium. Devergie attributes to the solvent power of water its agency in promoting putrefaction. A stream delays the change, probably by removing the putrid particles first formed, which, if remaining with the particles not decomposed, would hasten the process; and, at the same time, guarding the substance from the contact of air. Chlorine retards, by forming a white pearly compound almost imputrescible. Deutoxide of nitrogen has a similar influence by absorbing oxygen and sulphurous acid, by transforming the matter into a substance highly oxygenated" (*British and Foreign Medico-Chirurgical Review*, p. 396-7, vol. ii). "Stagnant water accelerates those stages of putrefaction entitled green, brown, and liquid. Putrefaction is less rapid in the water of privies. This is due, according to Devergie, to the presence of a large quantity of ammonia, which retards the liquid putrefaction, but favours the production of adipocere. He arranges the different media with reference to their power of favouring putrefaction in the following order: air, manure, the *water* of privies, stagnant water, running water, earth." (*Ibid.*, p. 400.) Smoke or carbon, in suspension in air, and creasote, have the same effect in arresting or preventing putrefaction. Animal matters in nitrogen, or chloroform vapour, do not putrefy, but will keep for an indefinite time, as shown by Dr. B. W. Richardson. Dr. Richardson also found that the presence of phosphorus in water, because it also absorbs oxygen, is also in great measure a preservative against putrefaction. Sea water favours decomposition sooner than fresh water, particularly, I believe,

when not in a concentrated state, *i.e.*, if diluted with much water, and of specific gravity 1010·15 instead of 1027, as is evidenced by the rapid putrefaction of sewage matters when admixed with sea water. Indeed, the foul odours of drains at low water in summer, especially if the pipes conveying sewage are not carried out far at sea, are proverbial.

I have said that changes of a fermenting character occur in the body normally. "Fermentation is, however, in amount small in the small intestines as compared with what it is in the large, and the reason is obvious; because the fermentation begun in the small intestines occurs with much greater rapidity in the latter, and is unimpeded by the presence of free acid." (Lehmann, *op. cit.*, ii, 130.) Marcet found that the constant reaction of fæces was alkaline. (*Op. cit.*) The experiments of Magendie, etc., confirm the statements made above. Magendie and Chevreuil found the following proportions in the intestines of executed persons :—

In the Small Intestine.

	Carbonic acid.	Nitrogen.	Hydrogen.
No. 1.	24·39 ..	20·08 ..	55·53
No. 2.	40· ..	8·85 ..	51·15
No. 3.	25· ..	66·6 ..	8·4

Chevillot found 2 to 3 per cent. of oxygen in the air discharged from the small intestines by aged persons. The hydrogen in these cases is generated by the amylaceous matters undergoing the butyric acid fermentation.

In the Large Intestine.

	Carbonic acid.	Nitrogen.	Carburetted hydrogen.	Hydrogen.
Magendie and Chevreuil	43·5 to 70..	18·40 to 51·03..	5·47 to 11·6..	
Chevillot (aged persons, 2 or 3 per ct. oxygen)	23·1 to 93..	95·2 to 90	..28·	
Marchand, No. 1.	36·5	..29·	..22·	..13·5
Marchand, No. 2. (1 sulphuretted hydrogen.)....	44·4	..14·	..15·	..15·8

Occasionally the amount of fermentation going on is excessive, as evidenced by the presence of copious frothy stools, and by the generation of unusual products. The changes here induced are thus explained by my esteemed

friend Dr. Ayres, in a private letter on this subject :—" The decomposition of fæcal matters would much resemble that of the food taken, and would differ according to the nature of that food. On the supposition that the animal had been fed on flesh, then a limited supply of oxygen must be afforded. The nitrogen would combine with hydrogen, forming ammonia. Part of the carbon, phosphorus, and sulphur, would also combine with the hydrogen, forming respectively carburetted hydrogen, phosphuretted hydrogen, and sulphuretted hydrogen, and another part of the carbon with oxygen, constituting carbonic acid. All these products, except the phosphuretted hydrogen, were found in the colon by Marchand."

But occasionally, also, the fermentation is one of *putrefaction*. Thus, says Lehmann, " substances stagnating in the different parts of the colon undergo complete putrefaction, and their products, gaseous as well as solid, are precisely the same as those we observe out of the body. In the examination of such masses Frerichs found substances precisely similar to those Bopp obtained from putrefying protein bodies." If it were otherwise, fæcal matter would be an exception to all other animal matters. Apart from the specific substances it contains, there are the usual proteinaceous matters and *débris* of food, which, under circumstances of heat and moisture, will putrefy.

But, further, we may glean something on the nature of such changes by reference to manures, and some of the changes which take place in them. My friend Dr. Ayres, in a letter to me on the subject, writes :—" When night soil is mixed with water, and distilled over by a very gentle heat, ammonia and sulphuretted hydrogen come over. If the distillated substance be now freed from sulphuretted hydrogen by distillation with carbonate of potash, and from ammonia by a second distillation with dilute sulphuric acid, a fœtid liquid is obtained, which gives a rose tint with nitric acid, and a beautiful lemon yellow when the acid liquor is supersaturated with ammonia; and if the original liquid be kept in a stoppered bottle, decomposition again occurs, and fresh ammonia is again generated."

The gases evolved in the atmosphere which surrounds common sewers may be likewise looked upon as produced by the putrefaction of fæcal matters. Thénard speaks of these as of two kinds : 1st. Atmospheric air, with hydrosulphate of ammonia in it. Thus, the air of one of the common sewers of Paris gave for composition, viz., oxygen,

13·79; nitrogen, 81·21; carbonic acid, 2·01; sulphuretted hydrogen, 2·99; besides a small quantity of ammonia. 2nd. The other gas is more rarely met with, and consists of —oxygen, 2; carbonic acid, 4; nitrogen, 94; sometimes mixed with ammonia.

These different products are evidence of a different kind of fermentation, or at least a different stage and progress of it. But the particular changes beside those dependent on the escape of ammonia in the *solid matters*, and in the production of fœtid liquid described by Dr. Ayres, I believe, have never been accurately investigated: the animal proximate and ultimate substances formed are not known. On the changes which *excretine*—the principle of excrement—undergoes, Marcet himself is silent. Such an investigation (because we have here offensive matters, if I might use the term, *in essence)* would be most important in its results, and would doubtless greatly reward the patience of any inquirer who would not allow his zeal for science to give way before the disgusting details and the offensive odours which would surround him. Dr. Ayres has stated to me he hopes some day to attempt the task. I hope he will be spared to unveil a few of the mysteries of this branch of chemistry.

CHANGES OF FÆCES IN DISEASE.

There can be no doubt that fæcal matters undergo very remarkable changes in disease, even *when yet in the body.* Independently of those which are visible to the naked eye and the microscope, as when blood, pus, pieces of fibrin, albumen, or other foreign matters, are detected in them, there are those of reaction and smell—sometimes intensely acid, sometimes neutral. But the smell is to me the most characteristic. If its occasional absence altogether be proof that the ordinary changes in the fæces have not taken place, the offensive odour is sometimes far more so—a smell so foul that even the ordinary odour of fæces, bad as it is, becomes in many cases a perfume compared to it. How frequently is this foul odour in them observed in infantile diseases, especially the *typhoid* varieties of exanthemata and remittent fevers, so characteristic as even to be noticed by the undaunted attendants of a sick room, and to be commented upon as a foul corruption, "smelling like a corpse". Indeed, the term *cadaveric* is the best epithet which I can find to express the character of this odour. Speaking of the smell in teething cases, Dr. Osborne, in a

paper on the Examination of the Fæces in Disease (*Dublin Journal*, xlv, 109), remarks:—"Sometimes it acquires a pungency almost intolerable, as is observed in cases even of slight diarrhœa, when, after the more solid contents of the colon and rectum have passed, they are succeeded by dark coloured fluid motions, consisting of fresh arrivals from the cæcum, of such overwhelming potency that even the most experienced and apathetic physicians are compelled to a speedy flight," etc. Dr. Osborne compares this odour to that of an ulcerated bony surface. Be this as it may, this peculiarity of odour is also noticed in many of our worst malarious fevers. In dysentery, this odour, according to Schönlein, is very characteristic. "In yellow fever," says La Roche, "in the first stages, and especially when obtained by purgatives, the matter discharged from the bowels has an offensive odour, emitting, indeed, in some cases, an intolerable and dreadful fœtor, alike repulsive to the sick and attendants." (La Roche on *Yellow Fever*, i, 263.) This cadaveric odour I look upon as evidence of the poisonous nature of such stools, as I shall show presently; and it is interesting to notice, in connexion with this advanced state of putrefaction, that the worst kinds of typhus are usually preceded by the most obstinate constipation. So also, "in the very large majority of cases of yellow fever," says La Roche, "constipation of the bowels constitutes a predominant feature in the early stage, proving in some instances as obstinate as in any known disease, and resisting at times the action of powerful cathartic medicines. This condition of the bowels is found to prevail more generally in the severer and more concentrated forms of disease." (*Op. cit.*, i, 260.)

INFECTION THROUGH FÆCAL MATTERS.

There are three conceivable ways in which disease may be generated by the absorption or ingestion of fæcal matters:—I. When in their natural, concentrated, undiluted state; II. When taken after dilution or suspension in water; III. When the emanations arising therefrom are inspired or otherwise absorbed in the system. Under these three heads I propose considering the subject. In doing so, however, it is right to premise, by the following considerations, which must be admitted *in primis*, otherwise the conclusions may prove unintelligible.

Infection, to be propagated, requires three concurrent

conditions:—1. The infecting source; 2. A transmitting medium; 3. A fit recipient.

1. *The source of infection* may be the person affected, fomites, his excretions, or, in fact, any personal part of him so contaminated. Such source is always more concentrated and potent at the *outset* of an epidemic, and the reverse towards the termination of it.

2. *The transmitting medium* may be *(a)* liquid, or *(b)* gaseous. *(a)* In the former case, as in the example of inoculation, it may be the blood; it may be water holding any poison in suspension or solution; and, in fact, any other fluid which does not destroy the poison, applied externally or taken internally with food or drink. *(b)* In the latter case, it may be a gaseous or volatile substance mixed or suspended in the atmosphere. Now, of these two varieties of transmitting agents, the *gaseous* and the *liquid*, the *barometrical changes which influence the development of the one may be the very reverse of those which influence the spread of the other*. Thus, the gaseous will be more energetic in hot and moist weather, and the liquid in moderately cold or temperate and dry weather; because, in hot and moist weather, the air is lighter, and the ammonia more volatile, which gas, with Robiquet, I believe to be a great adjuvant in the suspension of poisonous emanations in the air, especially when damp. Hence it is that in hot and moist weather many contagious diseases are rapidly conveyed and extended, whereas cold and dry weather will prove beneficial in its effect, the air being heavier, and the ammonia not only less volatile, but also less in quantity, because animal matters decompose less rapidly in cold weather. We are all aware how excessively fœtid our London drains smell just before rain sets in, so much so as to be the ordinary and popular prognostic of rain in this town. According to Robiquet, as I have said, the odour of gaseous (which may also be contagious) matter is due to the volatility of the ammonia, which, as it were, raises it to the nose. Ammonia is, moreover, always evolved in the putrefaction of organic matters. It is also very generally produced in cases of disease, and is always emitted in contagious diseases. (See Liebig's *Agricultural Chemistry*, by Playfair, fourth edition.) In cold and dry weather, since the air is heavier, and the ammonia therefore not so volatile, all poisonous matter will be more likely retained in solution or suspension in the flowing or stagnant waters; unless, indeed, the cold be so extreme as to destroy all fermentation. In this case, the poison may,

therefore, be more readily taken in the water, or other liquid ingesta.

3. *A fit recipient* is necessary: and here, even more than in the preceding conditions, there has been much error admitted. In judging of the effects of poisons, we have not fully considered the effects of *race* in man, *species* in animals, *concentration* of the poison if taken at the commencement or at the end of an epidemic, and the effect of *acclimatisation*, besides *previous diseases*, *occupation*, and *state of health* of the recipient. These are a few amongst many causes, the neglect of which has led to the most heterogeneous confusion and contradictory statements. Thus, a *negro* is less susceptible to fever than a *white* man. All *animals* may not be susceptible of taking certain diseases, or at least of catching them in the same way, as men do. Thus Beclard inoculated dogs with plague matter, and made them eat the matter from plague buboes and pathological specimens of plague; and they did not catch the plague. Similar experiments performed on criminals (*British and Foreign Review*, v, 561) communicated the disorder. Syphilis is another disease which cannot be in any way transferred to the lower animals. The symptoms may vary: thus, the dread of water in hydrophobia is a symptom invariably present in man, but not in rabid dogs. Again, small-pox may become cow-pox in a cow. The same man, *cæteris paribus*, as regards health, may, after exposure, get cholera at the commencement of an epidemic from its virulence, when, at the end of the same or a similar epidemic, from the milder character of the disease, he may get only diarrhœa. Then, in regard to the experiments on animals at the beginning of an epidemic, from the same cause, cholera may be induced by making them swallow the diseased egesta; and yet, at a later period of the epidemic, only diarrhœa will result. A person who has lived in an infected district for some time is less likely to suffer from an epidemic than a new comer; and so on. All these circumstances are most important to notice during contagious epidemics, and yet, I fear, are too often neglected even by our best authorities; and therefore I have thought it necessary to review them briefly, because they bear so manifestly on my subject.

I. EFFECTS OF ABSORPTION OF FÆCAL MATTERS IN THEIR CONCENTRATED AND UNDILUTED STATE.

Effect of Diseased Fæces on the Body. Healthy fæces, I believe, are not poisonous; and to some animals, owing to retained nutritive matters in them, they may prove nutritious, as in the case of those of the horse, partaken of by birds. I have seen the experiment voluntarily performed by dogs. The pointers and other *chiens de chasse* are notoriously voracious, and I have frequently seen them devour, apparently with great avidity, the fæcal matters in their way, and have never observed any peculiar effects produced therefrom; and other additional examples from barbarous nations, and hysterical and demented persons, might be adduced to prove the same fact. But, more than this, the annals of ancient medicine record their exhibition in the treatment of diseases. Fonseca (*De Excrementis Humanis*), quoting from Dioscorides (lib. ii, ch. 72) and Galen, enumerates a series of diseases, such as anginas, dysenteries, sore throats, phlegmonous inflammations, colic, many skin-diseases, etc., in which the fæces of man or animals were found curative in their effects. He has the modesty, however, to add that these remedies are better borne by unlettered rustics than by polished townsfolk. The record of such a practice, though eminently disgusting in the present day, is historically curious, and confirmatory of the non-poisonous properties of fæces in health if accidentally partaken of. This must be my excuse for alluding to the fact. On the other hand, many persons retain fæcal matters in their bowels for long periods with apparent impunity. The fæces in these cases are unchanged; no poisonous fermentation has occurred.

But it is not always so. Those who are acquainted with the works of Hamilton on purgatives and Burn on constipation are aware how far, in the opinion of these writers, the retention of fæcal matters in the bowels is capable of producing disease. In the hands of the former writer, typhus, scarlatina, marasmus, many nervous and chronic diseases, seemed sometimes to be produced, at least much aggravated, by accumulations in the bowels, and were greatly relieved or cured by aperients. According to Burn, it seems that the general health may be completely undermined by habitual constipation. There is scarcely a variety of indigestion, an uterine affection, a dysentery, or diarrhœa, not capable of being originated by

this cause. Without, however, asserting the universality or even generality of this cause, as productive of these diseases, it may be admitted that all these diseases may occasionally be so generated and kept up. In many cases, the effects produced are analogous to those usually observed to follow the absorption in the system of putrid animal matters, such as flesh, and pus in a putrefactive state. When these last are placed on wounds, vomiting, low continued fever, rigors, and death, often result. The danger of dissection wounds is proverbial. "The poison of bad sausages", says Graham, "belongs to this class of noxious substances. Several hundred cases are known in which death has occurred from the use of this kind of food. In Würtemberg especially these cases are very frequent, for there the sausages are prepared from very various materials. Blood, liver, bacon, brains, milk, meal, and bread, are mixed together with salt and spices; the mixture is then put into bladders or intestines, and, after being boiled, is smoked. When these sausages are well prepared, they may be preserved for months, and furnish a nourishing savoury food; but when the spices and salt are deficient, and particularly when they are smoked too late, or not sufficiently, they undergo a peculiar kind of putrefaction, which begins at the centre of the sausage. Without any appreciable escape of gas taking place, they become paler in colour, and more soft and greasy in those parts which have undergone putrefaction; and they are found to contain free lactic acid or lactate of ammonia—products which are universally formed during the putrefaction of animal and vegetable matters." This change of colour I have before remarked is observed in fæcal matter kept in a moist state and exposed to the air, and I presume it is indicative of an analogous change in it. Very lingering and remarkable symptoms precede death when this is the result of poisoning by putrefied sausages. There is a gradual wasting of muscular fibre, and of all the constituents of the body similarly composed. Other substances employed as aliments, as cheese, pork, bacon, mussels, salmon, in a state of partial putrefaction, occasionally produce poisonous effects. These vary, it is true, in degree and kind; but the effects are not the less certain. It is, therefore, in strict accordance with analogy to conclude that putrefying fæces may generate disease in the same manner, the poison being absorbed by the same intestinal membrane.

Moreover, that fæces are capable of inducing a putre-

factive fermentation in surrounding tissues, is evidenced by the character of the pus in abscesses about the large intestines. We all know how excessively offensive the pus in abscesses about the rectum and colon generally is—in fact, wherever fæcal matters are to be found in its vicinity; and this without necessarily, I believe, a direct communication. Here we have the ferment acting at a distance, perhaps through endosmosis, upon the pus, and exciting its decomposition, which is often acid in reaction, instead of alkaline as in healthy pus. Patients under these affections frequently suffer from typhoid symptoms and low fever. Their very expression denotes serious blood-disease. These symptoms, it is true, may be referred, secondarily, to the absorption of putrid pus; but they were no doubt due primarily to the changes effected in the pus. The habitual foul breath and tongue, the generally offensive, almost fæcal odour of some persons (particularly uncleanly females), who are habitually addicted to constipation, are evidences of absorption of fæcal matter under ordinary circumstances, which, like a contaminating drain odour (only originating in the patient himself), must prove injurious to the organism. The anomalous nervous symptoms originating in hysterical females and boys may often be referred to fæcal impregnation, so speedily are they relieved or entirely removed by a brisk purge. In these cases, we may suppose that the poisonous substance in the colon sets up a partial fermentation in the blood, which may in adverse circumstances excite fever, and even death.

Synocha or ephemeral fever is, I think, as frequently the result of such a change in grown up persons as remittent fever is in infants, yielding frequently to good purging, the motions having the abominable putrid odour before referred to, very different from that of healthy fæcal matter. These and many other allied diseases may be referred to this cause. The effect on the system is, however, certainly less violent than that produced by fæcal fermentation in water or in the atmosphere. If I might venture to call it so, I would describe it as poisoning only in the first degree.

Cholera. The experiments made with cholera excreta on man are necessarily few, and are almost exclusively confined to a few instances where love of science has overcome the natural disgust to such a course. Five such cases are recorded by Dr. Richardson, in his Review on the Water Supply in London (*Journal of Public Health*, vol. i,

p. 130). A sixth case was the result of accident. On Sept. 5th, 1853, four physicians tasted the intestinal fluid of a cholera patient. One of these (who, however, had been labouring under diarrhœa previously) took cholera on the 8th, and died in the evening; another was taken ill with choleraic symptoms on the 9th, but these symptoms passed into typhoid fever. An assistant in the Cholera Hospital at Berlin tasted choleraic fluid. Diarrhœa soon supervened, which passed into a smart attack of cholera. The accidental case is one related by Messrs. Pearce and Marston, in which a dispenser drank a quantity of rice-water evacuation by mistake, without any result. Dr. Richardson gives also the case of M. Scipion Pinel, who inoculated himself with cholera evacuation, and two hours afterwards suffered from sickness and vertigo, which disappeared after free perspiration; and that of M. Foy, who tasted vomited matters and breathed the air expired by a choleraic patient, but who, although unwell for four days, did not get cholera. In neither of these latter cases, however, were the fæcal matters tasted; and thus they do not bear upon this part of my subject. The other cases afford only negative and inconclusive evidence.

The lower animals have also been experimented upon with the dejections of patients affected with Asiatic cholera, but so far as I know, those of Thiersch of Munich are the only experiments made with putrid choleraic stools. I quote from Dr. Snow's excellent work on cholera. "Dr. Thiersch is of opinion that the choleraic evacuations are not capable at first of generating the disease, but that a decomposition takes places in them, and that in from six to nine days they become in a state to induce cholera. He founds this opinion on experiments which he performed by giving small quantities of the cholera evacuations to white mice." (*On Cholera*, p. 112.) From some experiments detailed at length by Mr. John Marshall, of London, in the *British and Foreign Medico-Chirurgical Review* for April 1853, we may, I think, conclude that choleraic disease, yet unaccompanied with the algide symptoms of Asiatic cholera, may be communicated to animals, by the ingestion of the cholera stools of man. In a paper by Dr. Lauder Lindsay (*Edinb. Med. and Surg. Journal*, lxxxi, 1854, page 275), it appears the specific disease cholera was not generated by feeding dogs on choleraic vomit and evacuations, nor even on the blood drawn during life or taken from cholera cases after death. The

effect was the production of a non-specific diarrhœa. Their general health did not appear to deteriorate, or the poison to act otherwise than as a predisposing cause to their suffering from cholera, after exposure to its exhalations. The diarrhœa generated appeared to be very violent, with griping, vomiting and cramps, and great exhaustion, but the expelled matters were always feculent. In two dogs the disease proved fatal. The *résumé* from the experiments detailed, both by Dr. Lindsay and Mr. Marshall, thus appears to be that in all these cases diarrhœa more or less severe, and vomiting were produced. This was the general rule. In some cases the symptoms were so severe that death resulted, but fæcal matter was expelled to the last. The *post mortem* examinations of these cases were, however, defective. Moreover, in the few instances recorded where actual cholera is said to have been produced and to have been in the end fatal, I do not find it stated whether the urine was suppressed or not; such a circumstance, had it been present, would have gone far in my mind to prove that this diarrhœa was, after all, cholera, modified in different animals. The experiments, so far as they go, are then inconclusive; besides, they certainly present no relation which is constant, to the quantity of fæcal matters swallowed; although, as a rule, a large quantity was more certain in its products.

I am not aware of any experiments that tend directly to prove that the evacuations of typhus and other malignant fevers have ever been experimented with in the same way. We may, however, reason by analogy that in some cases yellow fever is propagated by the evacuations. The black stools passed in many instances of this disease differ in nothing from black vomit; and black vomit has been known, even when rubbed externally over the body, to generate the disease.

The evidence from retention of diseased fæces is, however, more conclusive. I have already alluded to those bad cases of typhus and yellow fever which are preceded by obstinate constipation. Among ourselves, the most familiar example of disease which, *per se*, proves the poisonous influence of diseased or putrid fæces, is afforded in that variety of *puerperal mania* which is so often induced by accumulations in the intestines. It should be remembered that the female after parturition is placed in those conditions most favourable to absorption, as there has been loss of blood to a greater or lesser extent, and invariably as a concomitant more or less nervous shock. Absorption of any

offensive matters under these circumstances is greatly facilitated. Now these cases are generally accompanied by more or less head symptoms—the head may or may not be warmer, but a slight incoherent delirium at first, often passing into mania, presents itself. The face is sallow, and the conjunctiva is tinged with bile ; the tongue is remarkably foul, the breath offensive, often fæcal. The abdomen is very tumid and swollen, and in one or other of the iliac fossæ there is uneasiness, not to say tenderness, and on deep pressure frequently a large mass of fæces may be detected. The motions are peculiarly offensive, and in most cases emit that cadaveric odour which to my mind is so indicative of putrefaction in fæcal matters. The pulse is frequent, easily excited, from 100 to 120, and a shivering fit often occurs, thus denoting that the entire system is impregnated with the poison. The *locale* of the disease is often strongly manifested in the progress of the case. Rapid purgation may come on ; the head symptoms will be ameliorated, if not actually removed ; but the pulse will fall in frequency and increase in strength as the offensive matters are removed, an effect which, being generally the reverse of that observed after violent purgation, I look upon as almost diagnostic of this form of fæcal impregnation. It is clear in such cases that something more than fæcal accumulation is present here. The symptoms are those of a narcotic poison in its first effect with fever, and cannot but be partially explained if looked upon as due only to mechanical irritation. There are besides similar examples of disease occurring in non-puerperal cases, as in many instances of mania.

I have mentioned an extreme case, but the variety of minor affections of the same kind, differing only in degree, is legion. In these cases the poison is in the *primæ viæ* primarily ; and so long as it remains, so long will the disease be kept up.

Since writing the above, my attention has been directed by Dr. Richardson to a report by Dr. Rennie, in a parliamentary blue book on *Further Correspondence on the subject of Convict Discipline and Transportation*, of the results obtained by overfeeding convicts in Western Australia. These men receive upon an average from 7 lb. 8 oz. to 7 lb. 14 oz. of food daily, from 59 to 67 oz. of which were solid food. This gross amount of food resulted in the production of a disease which affected the individual in various

ways, sometimes bringing on dyspepsia, dysentery, severe constipation, or other analogous bowel complaints; sometimes a severe kind of ophthalmia; sometimes a cutaneous eruption. These diseases yielded to powerful purgatives and low diet. In one case (that of a man named Nain), the patient took 5 purgative injections, 480 grs. of compound jalap, 8 oz. of Epsom salts, 32 other purgative pills, 1½ oz. castor oil, 5 drops of croton oil, 8 grs. of scammony, and 6 grs. of gamboge, before the cure was effected. During this period, he voided 30 lbs. of feculent matter in a state of decomposition.

The daily average number of prisoners was 504; of sick, 42. The total number of sick in one year was 2,290; of whom 959 had diseases of the digestive organs; 598, diseases of the eyes; and 633, diseases of skin, including ulcers and abscesses. These diseases, as showing their origin from the same cause, were cured by the same mode of treatment and low diet, and were frequently metastatic one to another.

But what I wish to call attention to, is the fermentative character of the disease which was set up in connexion with the putrid character of the evacuations passed. Dr. Rennie says: "With respect to the peculiar nature of the blood disease, I stated that, from finding on a microscopic examination of certain forms of skin-diseases, a low form of vegetation, resembling the yeast plant (one of the most primitive illustrations of organic life), there were rational grounds for supposing that the large and badly constituted diet might lead to fermentation and the development of this low form of vegetation, which, after becoming absorbed and circulating for a time in the blood, ultimately might become expelled in the form of local disease. As a proof of the fermentative process going on in the intestinal canal, I mentioned that one of the most common appearances which the evacuations presented in the early stages of dysentery was that of copious, frothy, watery stools in a high state of fermentation, and bearing a general resemblance to yeast." (p. 119.) In a table given at page 128, where he records the cases seriatim, the amount of medicine taken, and the characters of the stools, the fermenting process going on is well exemplified. In 4, the fæces are described as being in a high state of fermentation; in 6, putrid; in 2 only, tolerably healthy; in 3, the matters voided were like pea soup; and in the rest, pus, blood, mucus, shreds of membrane, etc., existed in abundance. The number of cases so tabulated amount to 22, affected with ophthalmic and skin

diseases; half only, or 11, being affected with dysentery. This class of cases proves, I think satisfactorily, that fæces putrefy in the body if long kept there, and will give rise, if not removed in time, to serious blood disease.

PART II.

II. EFFECT OF FÆCAL MATTER WHEN TAKEN AFTER SOLUTION OR SUSPENSION IN WATER.

In entering upon the consideration of this subject, I feel bound to express the obligations under which, personally, we all lie to Dr. Snow, to whom the merit particularly belongs of having first called our attention to a source of disease previously ignored. Indeed, I find it difficult to express, in terms which I think would do justice to him, my sense of the importance of his discovery, and which he has also so ably enunciated, and proved, as I think, to demonstration, in his work on *Cholera*. Its truth is now admitted, with few exceptions, by most men of science. Besides, in its results, it has conferred already great advantages on the public, having originated the adoption of decisive measures in regard to the supply of water in this metropolis, and in other places. The results of a further development in this direction on the social condition of man may, like Jenner's discovery, be the means of preventing the spread of fatal disease, and saving thousands of lives to the nation. In this sense, I think our thanks to Dr. Snow cannot be too strongly expressed; and, for one, I feel I owe him a great debt of professional gratitude.

In speaking of *putrefaction* in the first part of this essay, I referred to the necessity for the presence of certain agencies to enable substances to pass into fermentation—*i. e.*, to absorb oxygen.

1. That *water* is sometimes necessary, is proved (among other circumstances) by the experiment made by Gay Lussac, who found that, if chloride of calcium were placed at the bottom of a jar of oxygen in which meat was suspended, the meat remained fresh many days; the chloride of calcium, from its affinity for moisture, keeping the gas in a perfectly dry state. Hay, straw, wool, if perfectly dry, will keep for any length of time; but, if moist, they will become slowly charred, undergoing a degree of oxidation which may pass into combustion if exposed to the atmosphere.

2. If, when water or moisture be present, a small quantity of fermenting matter be added, the process of fermentation will rapidly progress—much more rapidly than if time be given for its independent development. An experiment was brought before our notice, and detailed by Dr. Ayres, on the occasion of reading the first part of this paper, which applies so much to my subject that I cannot help again referring to it. Sir J. Pringle took the putrid yolk of an egg, into which he dipped a small thread. A bit of this thread was cut off, and put into a phial, with half the yolk of a new laid egg, diluted with water. The other half, with as much water, was put into another phial, and both, being corked, were put by the fire to putrefy. The result was, that the thread infected the first yolk very speedily; for the putrefaction was sooner perceived in the phial that contained it than in the other. *(Phil. Tran.* 1750, p. 554.) If, then, the ferment be thus supplied, the process in the water will progress most readily.

3. Water may become superoxygenated, both *(a)* by living animalcules and *(b)* by vegetable matters.

(a) By *animalcular life.* I have said before that oxygen is essential to bring about fermentation. Now water is, under certain circumstances, found to contain this gas in unusually large quantity. We all know that, while there exist in water certain kinds of animalcules which absorb oxygen and give out carbonic acid, thus exhaling a gas which, in course of time, will render that water unfit for animal life, so there are certain other animalcules which seem to act under the influence of light, as plants, giving out oxygen in large quantities. Thus (I quote from Liebig), Count Rumford noticed the fact some seventy years ago, that, if pieces of cotton, silk, and other organic substances, were placed in water, the water, after three or four days, became green, and was filled with minute spherical bodies, and evolved pure oxygen gas. No confervæ or plants of any kind were observed in this water. Again, "in the salt pans of the salt works of Rodenberg, in Hesse, a slimy transparent mass, which covers the bottom to the depth of from one to two inches, is found to consist entirely of such animalcules. This mass is everywhere interspersed with large air-bubbles, which ascend in great number through the supernatant fluid, when the pellicles enclosing them are torn by agitating the mass with a stick." "Pfanfuch, upon investigation, found this air to be pure oxygen gas, so that a wood splinter, the flame of

which had been just extinguished, rekindled in a flame when immersed in it. Wohler found that this mass consisted of living infusoria. Messrs. Charles and A. Worren, in the *Transactions of the Academy of Brussels*, 1841, showed that water, with the co-operation of organic matter, evolved a gas containing as much as sixty-one per cent. of oxygen; and that this phenomenon was to be ascribed to the presence of *glamidonas pulviculus*, and some other green and red animalcules belonging to a still lower grade in creation." The same fact was confirmed by Liebig himself, who, after filtering a water in a trough in his garden, coloured green by the presence of various species of animalcules, filtered it through a fine sieve, to get rid of all confervæ and vegetable matters; and then, by exposing it to the light of the sun, in an inverted broken glass, the aperture of which was confined by water, found that, after a lapse of a fortnight, more than thirty cubic inches of gas had collected in the glass, which proved to be so rich in oxygen that a glowing splinter at once burst into a flame in it *(Liebig's Letters*, pp. 240, 41, 42.) It is true that, in these waters, there must be the necessary conditions of life present, contained in solution or suspension in the water, so that this may become a fit nidus for the development of the germ, fissa, or ovule of the future animal; but, in any case, we here see one way in which water highly saturated with oxygen may more readily and more rapidly cause animal or vegetable dead matter to pass into fermentation.

(b) By *vegetable life*. At any rate, if animalcules of the kind just now referred to be not present, to explain this hyperoxygenation of water, it may be due to the presence of certain water plants which have the property of keeping water fresh and continually oxygenated, although replete with animal life, such as fish, snails, etc., and other animalcules which consume oxygen, and though the water be not changed. Such are the various kinds of *valisneria* and *anacharis*, which emit oxygen gas. These facts are well known in these days, when so many keep aquaria. Many other water weeds have the same effect. Now, it is clear that more oxygen will remain in this water when the atmosphere is heavy and the barometer necessarily high; and even in cold weather, from the same reason. In this latter case, the water, especially at some depth, is often hotter than the external atmosphere; and in both these instances fermentation will take place more readily in it.

There is also this point in relation to this excess of oxygen

in water, not to be lost sight of. The very excess of animal life (I allude to those animals which consume oxygen) is proof that this gas abounds to support this increase of animal life. But, even in those countries where we have no such index of animal or vegetable life to guide us, by reason of the intense cold, oxygen may be in excess, and in like manner its abundance will explain the prevalence of epidemic disease —Asiatic cholera, for instance—in Moscow and St. Petersburgh during winter. The atmosphere at that season, from its greater weight and density, contains more oxygen in a given volume; and the porous snow necessarily contains also an increased proportion. Here then is a great supply of oxygen, and fermentation is only kept back by the intense cold. On the other hand, two habits, which I believe are common to inhabitants of snowy regions, assist to bring about this result. One is, that, owing to a want of proper water-closets, the slops are generally emptied in the neighbourhood of the houses. The other is, that, owing to the expense of fetching water from the river, and the natural unwillingness to face the intense cold longer than is absolutely necessary, the inhabitants prefer collecting the snow around their houses, and melting it down for drinking water; and thus water impregnated with fæcal matters, or at any rate very impure, is drank; and hence, the moment it meets in the alimentary canal, or in the hot houses, the circumstances favourable to fermentation, this process is readily set up. Owing, moreover, to the excess of oxygen, it is very rapid when once it has begun, and thus any disease so induced becomes speedily fatal. To talk of emanations at temperatures as low as zero, and below that, which may be sensible to the organism, is, I deem, unphilosophical; and if we deny contagion to be in operation, the only place in which those circumstances of heat and moisture are present, and in which they may be developed, is in the alimentary canal, and through the ingesta introduced. So far as I know, this is the only reasonable explanation that can be afforded.

But this influence of a dry state of the atmosphere is not confined to cold weather. It has long been noticed in summer, although, so far as I know, not sufficiently insisted upon. Experience proves that, in some periods of the greatest intensity of an epidemic, the weather has been dry, and the barometer high, and the atmospheric temperature not necessarily low. These circumstances are, as I before stated, precisely those most likely to contaminate the water

and eatables taken by a population; and the reverse of those which favour the evaporation of poisonous miasmata, namely, heat, moisture, and a fall of the barometer. It probably explains the reason why the Thames water is more fatal in dry weather with a high barometer, and this at a period of autumn when the temperature is not so elevated. To this fact also the immunity of some low districts may occasionally be referred, when the high suffer most. The poisonous particles, otherwise suspended in the atmosphere by the ammonia and moisture, are kept to the ground, and thus become part and parcel of the water we drink. It is remarkable how this dry state of the atmosphere was observed in the London epidemics of cholera. Thus Mr. Glaisher remarks: "In the year 1854, the pressure of the atmosphere was very great; the temperature generally high; sky overcast; direction of the wind, north-east and south-west; and the velocity of the air was less by one-half than its average for some time before; and, at the time of the greatest mortality from cholera, the barometer reading was remarkably high, and the temperature above its average. A thick atmosphere, though at times clear, everywhere prevailed; weak positive electricity; no rain. In low places, a dark mist and stagnant air, with a temperature in excess; temperature of the Thames very high; a high night London temperature; a small daily range; an absence of ozone; and no electricity." (*Board of Health Reports.*)

The three epidemics of 1832, 1848, and 1854, were attended with a particular state of the atmosphere, characterised by a prevalent mist, thin in high places, dense in low. During the height of the epidemic, in all cases, the reading of the barometer was remarkably high, and the atmosphere thick. In 1849 and 1854, the temperature was above its average; and a total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion (*Journal of Public Health*, No. IV, December 1855). As if, moreover, denoting the absence of ammonia in the air, Dr. R. D. Thomson mentions that the air in choleraic wards as well as the external air was *acid*, and alkaline only in sewers.

4. There is also a general remark which will explain the exemption of certain parts, even though these were possessed of waters containing the same general excess of oxygen. I believe, in London—and here I speak especially of the West End—that one of the reasons of the innocuity

of these waters was their alkalinity. My attention was forcibly called to this point by the case of a patient for whom I had ordered on one occasion the infusion of roses. I was soon afterwards sent for in a hurry to see my patient, who was supposed to have been poisoned, the lips and tongue having assumed a bright yellow brown colour. The medicine had been taken, and afterwards a glass of water. The whole was explained on testing the water, which was found very alkaline, owing to an excess of *lime*. The water in Dorset Square and Montague Square, at my own and my patient's house, as well as that of the Bryanstone Square pump, was strongly alkaline from the same cause. This innocuity of the water, even if it contains fermenting matters, is analogous to the fact observed by Pariset in Egypt. Pariset believes that the reason that the overflowing of the Nile proves so salubrious in its effects is, that its waters are *very alkaline;* and hence, as it spreads over the land, it reaches the dead, and temporarily neutralises the miasmata; and thus the plague is stayed (*British and Foreign Medico-Chirurgical Review*, p. 249).

If such be, however, the case with water containing excess of alkali, it is not so with sea water, especially if much diluted with fresh water, or fresh water with chloride of sodium. An experiment of Pringle proves that sea salt, *if in small quantity enough*, favours putrefaction. "One drachm of salt preserves two ounces of fresh beef in two ounces of water above thirty hours uncorrupted, in a heat equal to that of the human body; or, what amounts to the same thing, this quantity of salt keeps fresh meat about twenty hours longer sweet than pure water. But half a drachm of salt does not preserve it above two hours longer. . . . Now, I have since found that twenty-five grains have little or no antiseptic virtue, and that ten, fifteen, or even twenty, manifestly both hasten and heighten the corruption. It is, moreover, to be remarked, that, in warm infusions with these smaller quantities, the salt, instead of hardening the flesh as it does in a dry form in brine, or even in solutions such as our standard, here softens and relaxes the textures of the meat more than plain water, though much less than water with chalk or tartaceous powders" (Pringle, *Philosophical Transactions*, 1750). Pringle found, moreover, that, of the quantities ten, fifteen, and twenty grains, in two ounces of water, the former was the most putrefying in its effects. Now, sea water contains between twenty-five and twenty-six grains in two ounces;

and therefore, if diluted with more water, as where the sea is in connexion with large rivers, so that the proportion of sea salt is still further diminished, it will putrefy much sooner. Hence, perhaps, the reason that cholera is more rife along seaports, where large rivers are also present, because putrefaction is more rapid. This view explains, moreover, the following commonly observed fact. It is known that a tough duck or curlew will eat very tender if steeped over night in weak brine or sea water. Tenderness of meat denotes incipient decomposition, although to a small degree. Fresh water will not, except in very warm weather, produce a similar effect. I have verified the same point by another experiment. Sea water from Brighton will keep longer sweet in stoppered bottles than sea water from Southend. Some of the former, collected on April 20th, of specific gravity 1018, was fresh up to the 10th June; some of the latter, collected May 12th, of specific gravity 1022, was putrid long antecedent to the 10th June. This is not surprising. The Southend waters, contaminated more or less by the sewage matters of the Thames, and yet containing less sea salt, would be, *cæteris paribus*, more readily decomposed. Besides, all sea water contains a large quantity of sulphates—from four in our channel to seven parts per 1000 in the Mediterranean; and these salts, in contact with animal matter, readily decompose, giving out sulphuretted hydrogen.

5. The existence of a poison in the water drunk may be inferred from the following considerations.

There must be in many cases something more than an emanation or miasma to explain the recurrence of some epidemics. The mere stench or overcrowding of a locality will not always account for the spread of disease. Take the cholera epidemic at Berlin in 1853. "In Prenzlau Street and Gollnow Street, in Berlin, there are a great many overcrowded cottages, and yet the number of cases here was small. The solution of the puzzle is all the more difficult when it is considered that the houses in Büsching Street, Wine Street, and in front of the new King's Gate, stand much more open than the rest of the houses of the whole of the (ninth) medical district. There can be no question here of confined space and want of fresh air—circumstances which are usually supposed to favour the origination of cholera; and yet there were four to five times more cases here in proportion to the number of the inhabitants than in the narrow Gollnow Street, in which the circulation of air

is much impeded. . . . . It certainly seems extremely surprising that, of the forty streets which compose the medical district, precisely the most openly situated of them were those which were most visited" (*Journal of Public Health*, vol. i, p. 280). Now, this something more is often, without doubt, *bad water*. Thus Scilla, a small village about fifteen minutes distance by sea from Valetta, in Malta, has always escaped cholera, while it has prevailed extensively in every other part of the island. The reason is this, all Malta (except this little village, which is supplied by tanks) is supplied by an aqueduct. And similar cases abound.

6. The *Board of Health Reports* show that the air was, during these cholera epidemics, in a weak *positive* electrical state. This, as before said, is precisely that state which attracts oxygen, and, as such, is most favourable to decomposition. It is to be regretted that the condition of the water was not also given. There is room in this direction for much inquiry. A blast of air, a direction of the wind, may determine the poisonous change in water: north and east winds bring usually positive, south and west bring negative electricity. Indeed, Dr. Bressler, in reference to the Berlin epidemic of cholera before referred to, proposes the question, whether, under certain circumstances, the admission of fresh air may not be prejudicial, as actually being the bearer of the miasma. The meeting of an epidemic may thus be synonymous with the meeting of a pernicious current—say a positive electricity, which developes the latent poison; and explains in this manner the healthiness of one place to-day and its unhealthiness to-morrow. Certain atmospheres, as well as waters or subsoils, in themselves harmless at one time, may, by virtue of their being the fittest nidi for the development of ferments, very rapidly multiply these, the moment they come into juxtaposition. This opinion is well set down by Surgeon J. H. Kerr Jones, of the 56th Regiment, in his evidence on the Bermuda fever (p. 62):—"When fever prevailed to a frightful extent among the troops in Upper Scindh, I have often at night been exposed with impunity for hours whilst wild fowl shooting, at what we considered the most concentrated sources of the poison, whose effects, even when infinitely diffused, were supposed to be deadly. The neighbourhood of such situations remained free from any disease. The troops who marched over them, and daily encamped there, continued in the enjoyment of health: it was *only when the epidemic influence prevailing*

on the rock of Jukhur was encountered, that every *drain* and *cesspool*, or room abounding in exhalations from the human body, became impregnated with its poisonous property."

7. It is probable that the *chemical rays* of light may in some way be concerned in the development of oxygen. According to recent experiments, these chemical rays vary much in quantity. A solution of quinine (the photographic test) becomes quite bluish and milky looking when these abound, and quite transparent when they are few in number; and hence the reason that photographs are so easy of execution at one time, and so difficult, if not impossible, at another. The effect of chemical rays on putrefaction has not, so far as I know, been studied; still, some facts may exemplify how an effect may be produced on them. A solution of chlorine in water, if exposed to light, will decompose the water, and liberate the oxygen. Some chlorides have an opposite effect. Thus, freshly precipitated chloride of silver will also, if exposed to light, decompose water; but the oxygen set free unites with the silver, forming oxide of silver; and thus the liquor assumes a black colour. Of the several rays of the spectrum, the violet produce this change more rapidly than the red on the moist chloride of silver. "Their characteristic effect is to promote those chemical decompositions in which oxygen is withdrawn from water and other oxides; and hence they are sometimes called *deoxidising* rays." (Graham's *Chemistry*, 1st. ed., p. 93.)

Recent inquiries, at any rate, prove that there is a connexion between these chemical rays and the electrical currents induced. How far, then, these two causes are co-operative, remains to be shown; but that they are often concurrently present, and in their effects materially influence decomposition, cannot be denied.

The seven conditions enumerated above all produce decomposition; and we may thence, I think, infer that, although their coexistence may not always be necessary for the development of every epidemic, their coexistence may materially increase the power of its action, as their disunion may diminish its intensity.

I proceed now to consider the effect of fæcal fermentation, in solution or suspension in water, in the production of *particular* diseases.

Cholera. It is not my intention to dwell at great length

on the subject of the spread of cholera by water impregnated with choleraic dejections. I conceive it is now generally admitted by all that cholera is most prevalent and fatal in the course of large rivers, and where the water supply is bad. Dr. Snow's work is full of examples on this point. I shall take from this work only three such examples, those of Newcastle-on-Tyne, South London, and Golden Square.

Newcastle and Gateshead. The following table exhibits the mortality from cholera in the periods 1851-2, 1849, and 1853 :—

Newcastle.

Year.	Actual mortality.	Population.	Deaths to 10,000 population.	Remarks.
1831-2	801	42,760	187	No water works.
1849	295	71,847	41	Good water supplied.
1853	1533	86,114	178	Bad water supplied.

Gateshead.

1849	Comparative immunity			Water works and water good.
1853	433	26,000	166	Bad water.

Both Newcastle and Gateshead are supplied by the same water company. Previously to 1832, there were no water works; subsequently, these were made, and water was obtained from the Tyne, about a mile above the town, although the tide flows six miles upwards beyond it. In 1848 these water works were abandoned, and excellent water was supplied from a small stream ten miles above Newcastle, called Whittle Dean. In 1853, the water from Whittle Dean being insufficient for the wants of the town, the original water works of 1832 were reopened, and thus bad water mixed with the good, so bad as to contain 7·1 parts of organic matter per gallon. (Dr. Thomson says at its origin there was 15·6 total impurities, of which 4·5 were mechanical, 2.68 organic in solution, and 8·48 organic impurity.) Thus, according as the water supply was good or bad, so the mortality from cholera was low or high. Again, the greater number of deaths, viz., 1011 out of 1553, occurred at Newcastle from 13th to 23rd September, inclusive. The reason was this. Owing to the outcry, the company supplied, after the 15th, no more Tyne water; and although

the Tyne water was not out of the pipes till the end of a day or two, the deaths decreased from the 19th.

Sept. 12th	38	Sept. 18th	103
„ 13th	59	„ 19th	111
„ 14th	90	„ 20th	85
„ 15th	106	„ 21st	68
„ 16th	114	„ 22nd	82
„ 17th	103	„ 23rd	60
	510		509

It is also to be remarked that places supplied with pump water, and not with that of the company, at most suffered from simple diarrhœa, and not from cholera. Thus, in the workhouse, supplied by the company, out of 440 inmates, the number of deaths was 7 out of 12 cases of cholera. In the barracks, with 590 inmates, and in Greenhow Terrace, supplied by wells, there was no cholera, only diarrhœa. (See Dr. Snow's work.)

South London District. This district was supplied with water from two sources, the Lambeth Company and the Southwark and Vauxhall Company. In 1849 the former company got its water from the Thames, near Hungerford Market; the Southwark and Vauxhall from the same river, near Battersea fields. In 1854 the water of the latter company was obtained from the same place, while the former procured it from Thames Ditton. In 1849 the mortality was nearly the same in the districts supplied by each company. In 1853, the deaths were, in those parts supplied by the Southwark and Vauxhall Company, 525; while only 94 occurred in those supplied by the Lambeth Company, and 33 in those districts supplied from pump wells, and other sources. This mortality being divided over two periods, the first four weeks give 286 cases against the Vauxhall Company to 14 against the Lambeth; while, for the remaining three weeks, the deaths were in the proportion of 8 against the former to 1 against the latter. In the General Report of the Board of Health, speaking of the supply of water in London, is the following paragraph: "Those supplied by the Southwark and Vauxhall and Chelsea Companies greatly surpass the others in badness. In the Southwark and Vauxhall water, the evidence of unfiltered contamination reaches its highest degree, revealing to the microscope not only swarms of infusorial life, *but particles of undigested food, referable to the discharges from human*

bowels." (p. 46.) And it is this last contamination which propagates especially the disease, and which is plentifully supplied by the admixture of sewage matter.

Golden Square. The last example to which I shall allude is that afforded by the Golden Square tragedy. As Dr. Snow remarks, "The mortality in this limited area (*i.e.*, within two hundred and fifty yards of the spot where Cambridge Street joins Broad Street) probably equals any that was ever caused in this country, and it was much more sudden, as the greater number of cases terminated in a few hours." (*Op. cit.*, p. 38.) Upwards of five hundred deaths occurred in ten days. I cannot follow the history of the cases, which have been so ably drawn up by Dr. Snow. I can only allude to a few points. The outbreak commenced on the night between the 31st August and September 1st, and was to be traced to the drinking of water from a particular pump in Broad Street. In the subdistrict of Golden Square, Berwick Street, and St. Ann's, Soho, the deaths registered for the week ending September 2nd were 6 in the first four days, 4 on Thursday 21st, and 79 on Friday and Saturday 22nd and 23rd. Into those occurring in these last three days, 83 in all, Dr. Snow made close inquiry. In 68 of these cases the patients had all partaken of the pump water. In a coffee-shop in the neighbourhood, frequented by mechanics, and supplied with this water at their dinner, by the 6th of September ten of its frequenters had died of cholera. It was partaken of by a gentleman and his brother in Poland Street, both of whom died. It was partaken of by a lady and her niece, living at Hampstead, who sent daily for this water from preference, and both died of cholera. Two remarkable instances of comparative exemption in the same locality of persons not partaking of this, but of other water, prove the same truth conversely. In the workhouse in Poland Street, out of 530 inmates, only 5 had cholera; the other cases of cholera were brought from without. Here the Broad Street pump water was never partaken of; but there was a well on the premises, besides a supply of water obtained from the Grand Junction Water Works. Secondly, in the brewery in Broad Street, out of 70 workmen employed, only two had slight attacks of cholera. The reason was, they drank little or no water, but malt liquor, and, besides, there was a well on the premises. The pump water was, then, the cause of this cholera; and it has now been shown that, on the day preceding the outbreak, a boy had had

cholera in an opposite house, the water closet pipe of which passed about three feet from the well pipe, that the mortar work between the two pipes was defective, and that thus there was a communication allowing admixture of their contents.

So far, therefore, as these facts go, they appear to shew that the choleraic matters in solution in water do communicate the disease. But the mass of other evidence in the same direction, recorded by Dr. Snow, has proved this to demonstration. The immediate poison being a ferment. hitherto undiscoverable, because too minute even for the most powerful microscope of the day, Dr. Snow calls it a cholera cell; but, after all, the action of this cell is the same as that of a ferment. It is a distinction of words without a difference in meaning.

The examinations of waters in choleraic seasons may be thus detailed; and I have adopted a tabular form especially, as bringing the points of difference between the water from our companies, or from wells, or pumps, more saliently forward.

Water Companies.	*Pump Water or Wells.*
1. Contained organic matter, dead and living, annelida, entomostracea, infusoria, confervæ, dromidaceæ, diatomaceæ, *fungi*, hairs of animals, starchy matter, dead and decaying fragments of vegetable tissue.	1. The same, excepting hairs and starchy matter, among most wells. But *deep* wells, and spring water, were remarkably free from organic matter. In some there was total absence. In a few, as Wadden water, and Painham water, there were a few confervæ, dromidaceæ, and diatomaceæ, but no *fungi*.
2. No animal peculiar to cholera was found. One kind only gave room for suspicion, but it has been found since in harmless waters; viz., a brown, *fat*, *lenticular*, moving, spiral-like body.	2. *Ibid.* This body was also found in the Golden Square pump water.
3. Vibriones also abound.	3. The same.
4. The purest of the metropolitan waters was that of the Lambeth Water Company; the impurest was that of the Southwark and Vauxhall. In the latter, there were visible quantities of fæcal matters.	4. In some sewage matter was found. In the water of two pumps especially in Putney—in Price's Folly and Creek Buildings.

5. In some of the water of the Southwark and Vauxhall, Lambeth, and West Middlesex Companies, animalcules were found, which exist only in brackish waters, showing admixture of sea water, viz., the tritrychia trigma, melosura nemuloides, and coscinodiscus eccentricus.	5. Not found.

Typhus and Typhoid Fever. These are two diseases which have for centuries infected this country, and which may, I fear, be traced to a similar origin—cesspool water. In proof of this, I shall first adduce the example of the fever at Hastings, which I before detailed to the Society. The first case occurred in a large room in the uppermost story of one of the largest and best ventilated houses in St. Leonard's, on the Hastings side. There does not appear to have been any other cases of typhus in the place before, and I am also bound to add that this patient had not, so far as could be collected from inquiry in this point, been exposed to typhoid contagion. In this house, however, a woman—the cook to the family, and the inhabitant of this upper room—was seized with fever, which soon passed into malignant typhus. Owing to insufficient attendance, and by reason of the water-closet being placed two stories lower down, the evacuations from this woman's bowels, which were fearfully offensive, were frequently retained in the room for nearly a whole day before removal. The crisis in this fever was, in fact, a copious diarrhœa, during which the pulse fell from about 160 to 90 in the course of a few hours. Six persons who were living in that house caught the fever, and one gentleman died. All had feverish symptoms, accompanied with copious vomitings; though, in some, the disease fortunately stopped there. I learnt, subsequently, that after the house had been purified, two new visitors, who took the lower rooms, were also seized with fever. The origin of the disease was traced to a direct communication betwen the cistern of the water drunk by the inmates and the pipe from the water-closet. As the house was beautifully ventilated, and there was no communication between many of those who caught the fever, it is most probable that the disease was produced in the sick by the drinking of the polluted water. A similar result has been observed in other cases of typhus. My information, however, on this subject is necessarily meagre. I applied to

Dr. Corrigan, of Dublin, to know whether such an effect had ever been noticed in the Richmond Fever Hospital. "In a fever hospital", he remarks, "there is such cleanliness observed, that there is little exposure to the action of fæces, and the purity of the water supplied would certainly give no room for this source of contagion." The Croydon typhoid fever, which proved so fatal, was due to the water being impregnated with drains of a very fœtid character. This fact has been equally dwelt upon by Drs. Snow and Richardson. Croydon is gravelly in its soil, and thus porous to the transudation of liquid matters. The place, moreover, like most country places, abounds with cesspools. Up to the beginning of 1852 the water supplied was obtained from shallow wells and stagnant ponds. In 1852 these were abandoned, and the water supplied from a deep well, dug fifty feet in the chalk, worked by a steam engine. Towards the end of 1852, this supply was interfered with, owing to the new drainage works established by the Board of Health; the inhabitants had recourse to the old pumps, wells, and ponds, the former contaminated with cesspool matters, and the result was a virulent epidemic of typhoid fever. In the following account of an epidemic, quoted by Dr. Jenner in his Gulstonian lectures, the same origin probably existed. The facts are given on the authority of Dr. Hunt, of Buffalo.

At North Boston, Erie County, U.S., in 1843, resided nine families. Taking a tavern for the centre, seven of the nine lived within an area of one hundred rods in diameter. All the inhabitants, with the exception of the members of one family, were in the habit of frequenting the tavern. A feud existed between the master of that one and the tavern keeper. A man labouring under typhoid fever—a disease previously unknown at North Boston—took up his residence at the tavern September 21st, and died October 29th. Between October 19th and December 7th, twenty-eight persons in this little community had typhoid fever. Three families only escaped the disease, viz., the two residing the farthest from the tavern, and that of the man who had a quarrel with the tavern keeper, and, consequently, never visited at his house. Now, a fact of interest in this case is, that all the families in which the disease appeared drew their supply of water from the well of the tavern, while two out of three that escaped had their water from other sources. The man at feud with the tavern keeper was accused of having poisoned the well of

the tavern. He resided nearer than any of the others to the tavern. None who visited the village simply for the purpose of rendering assistance to the inhabitants contracted the disease. (Gulstonian Lectures by Dr. Jenner, *Medical Times*, March 12th, 1853, p. 26)

It is true in this case, the necessary link, that the first patient's fæcal matters had been mixed with the well water, is wanting; and yet it seems to afford a probable explanation. Moreover, when we consider that the well and the cesspool are generally, in country places, in the same yard, and often close to one another, the admixture may sometimes occur through a porous soil, and in this manner occasionally explain the occurrence and spread of disease. Dr. Sutherland (*Report*, p. 19) remarks, "Much of the evil resulting from the close proximity of rivers and canals proceeds from the infiltration of the subsoil, and not merely from the aqueous vapour, which rises from the surface of the water itself." Speaking, moreover, of the Thames, he says, "Besides the evil resulting from imperfect declivity, the sewers are back watered at high tide, and actually become the means of distributing a polluted and unwholesome drainage through all their ramifications, by which the whole subsoil becomes infiltrated with impurities." It is also clear that any imperfection in a sewer, a broken brick, for instance, in connexion with a similar imperfection in a pump close by, may lead to the drinking of contaminated water by a whole neighbourhood. Now, I have remarked that such waters may lose all smell, and even appear to be quite clear, and yet be poisonous from matters held in solution. I remember the example of the common sewer of Fontainebleau, which passes through the park, which is of a gravelly and rocky soil, and which, at times, after about half a-mile's course as a rivulet, is in appearance perfectly clear, limpid, and inoffensive in odour.

The same fact is mentioned by the Committee on Dysentery of the American Medical Association (vol. x, p. 568). Speaking of Seconsia, in Calhoun County, it is remarked that the water in the streams was very low, emitting an unusual smell from a quantity of decayed fish having been thrown in it in July. A long time after *men* ceased to detect any offensive odour in it, some *horses* would not drink of it below the bridge, but hurried above it, and drank heartily of the stream there. The first case of dysentery occurred in this neighbourhood. Mr. Foote, in Drs. Arnott and Kaye's *Report on the Sanitary Condition of*

the Labouring Classes in and about the Metropolis, remarks, "Two years ago a fever raged at Red Hill, which I attributed to the lodgment of the filth from privies, which I had removed at the time; the same thing occurred also at the Hyde, the fever prevailing there being of the typhoid type. Again, another medical officer writes that fever has been most prevalent in those courts and alleys where there is no free circulation of air, such, for instance, as Rosemary Lane, in which there are about twenty houses, in almost every one of which fever prevailed. The disease first made its appearance there in the month of August last; and, on my first visit, I found the intolerable nuisance of the overflowing of a cesspool, or privy, which continued for some time, there being no sewer to carry off the soil." (p. 4.) Now, a very little reflection explains how a sudden shower of rain would cause these fæcal matters to be carried down in the neighbourhood of wells, so that a very contaminated and impure water came to be drunk, through which fæcal matters might be absorbed in the system. Such may also be the case in Munich.

Diarrhœa is, as is well known to many, a disease frequently generated by the drinking of water contaminated with sewage matter. Indeed, where organic matter in solution in water abounds, this effect is usually produced. This was the case in Croydon, where a severe diarrhœa in the Park Lane School was traced by Messrs. Carpenter and Westall to the admixture of sewage matter with the well water drunk by the inmates. The frequency of diarrhœa in choleraic times, from the drinking of bad water, is also proverbial, such water containing offensive matters in solution, mostly derived from night soil, or other refuse matter. This same effect is commonly produced by the water of many of the inland lakes of Lower Canada, in America. These are, for the most part, very deep, and abound with fish of all kinds, whereas the rivers which issue from them are very narrow, so that the surface only of a lake is removed. The waters are generally brown in colour. When a boy, I remember, half a tumbler of the water of one of these, about fifty miles from Quebec, in the summer season, was, in our happy ignorance of the cause, the usual aperient —no doubt the animal, vegetable, and fæcal matters, in solution, contributing, along with other organic matters in a state of fermentation, to produce purgation. The effects, in many instances, resemble those occasionally produced by vegetable matters in particular states of

decomposition, also in solution in water; and probably hereafter it may be found that in both cases the ferments are identical—protein bodies, such as caseine, albumen, and gluten, which also produce analogous diseases. The following example, taken from Pereira's *Treatise on Food*, illustrates this point. In the course of a trial for damages, at the Nottingham assizes, it was proved that a very severe epidemic of dysentery, which had occurred among some cattle, was caused by their having partaken of water impregnated with the putrescent vegetable refuse from a starch manufactory. All the fish and frogs contained in it died. The amount of disease in the cattle, moreover, bore a direct proportion to the amount of putrescent organic matter contained in the water, and taken by the animals. The mere putridity of the water could not have produced the disease. The water of ponds in fields is often very putrid, and yet full of frogs and fish, and the cattle partake of it with impunity. There was, therefore, some ferment, most probably gluten, in this refuse, which, developing in the water a poison, generated disease on all who partook of it. This is the only explanation which offers itself; otherwise we must suppose that, the fæcal discharges of an affected animal having first mingled with the water, the water fermented the morbid principle, and the disease spread in consequence; and this, after all, is not an improbable supposition. This effect is analogous to that kind of diarrhœa produced in infants whom it is attempted to bring up by hand, and in whom the food, whether animal or vegetable, passes off in a state of putrefaction in most offensive stools, the fœtor of which it is almost as difficult to describe as their production is difficult to restrain, even by the most energetic medicines.

Dysentery has been proved, in many instances, to have been produced by fæcal matters in solution. The previous case, detailed under the head diarrhœa, as an instance of the second supposition, is correct. Bad water is a frequent cause, at any rate, if contaminated by organic matter in a state of decomposition. Dr. Snow alluded, in his book on *Cholera*, to the occurrence of dysentery from bad water in Millbank prison. Dr. Bryson mentions cases in which dysentery was produced by the waters of the Yangote Kiang, the Canton river, and other rivers in China—and in which nearly all the patients were affected with great numbers of lumbrici. Zimmermann describes this appearance of worms in the evacuations of the patients affected with dysen-

tery in the epidemic of 1765, in Switzerland, as a very bad sign—*signe funeste.* These worms were no doubt taken as ova into the system chiefly in water, and afterwards developed in the body. A fact came to my notice, in which two deaths occurred from the use of impure water at Filey in Yorkshire. The drainage at Filey, as late as September and October last, was very bad. It is the practice of the fishermen also to wash their fish in water within the town. These drains, at certain periods, therefore, in their downward course to the sea, emit a fearful odour. At other times, however, it is not so. On one of these latter occasions, when this sewage water was free from smell, through the carelessness of a domestic, a portion of it was administered to a family, and two deaths resulted very speedily. The symptoms were magnified by some into cholera; but, from all I could learn, they were rather those of dysentery. The other members of the family also suffered, but the effects produced were, fortunately, not fatal. From what I have been able to learn on the subject of the dysentery in the Crimea, I think the presence of drain or fæcal matter in solution in the water drunk had a great deal to do with the production of dysentery. It very readily yielded to the drinking of bitter ale; negatively, I presume, by the substitution of an uncontaminated drink for water, which, through the rainy season, washing down the fæcal excrementations of a camp, must have been more or less impregnated therewith.

Plague, although, in most cases, to be traced to emanations from dead bodies in those cases where it appears to have been generated sporadically, is occasionally, so far as we know, produced by solution of sewage matter in water in those countries where the disease is usually rife, and where it might be said to be endemically prevalent. The waters, in a low district, are, for this very reason, more deleterious in a country without common sewers than in higher districts, where the admixture is not so easy. The observation that fæcal matters are capable of retaining the poison of plague, belongs to an old writer, Mercurialis; he states distinctly, that the poison of plague may be preserved in the excrements, as well as in clothes, and this without any actual injury to patients themselves, but that it cannot be preserved in living matters. (*De Peste*, p. 54.) Cairo is, moreover, a hotbed for plague. Now, in the midst of this town there flows a large open canal, or common sewer, —for it is nothing else, if we look to the carrion and fæcal filth it contains,—and yet as the Nile overflows, and mixes with

this sewer, portions of it are drunk with the water, and this gives rise to plague. And it is remarkable to notice how fearfully this disease spreads where the supply of water is bad or scarce, and where, necessarily, the water becomes contaminated the more readily. Such was the case in the fearful epidemic which broke out in Malta in 1813, and which never yielded to the most severe prophylactic measures that could be adopted, but was only stopped by the compulsory removal of the inhabitants into another part of the island, where we may suppose their food and drink were no longer contaminated. Indeed, to non-contagionists, the above is the only practicable explanation.

Yellow Fever. In regard to yellow fever, and its origin from bad water, there are, perhaps, no very strong facts to prove it. Still there are some negative facts, which may give a colour to it; one in particular, to which my attention was drawn in reading Dr. Richardson's able paper in the *Journal of Public Health* on the subject of Water Supply in this Metropolis, and from which article I have largely drawn in the argument of this paper.

In the year 1853, a very fatal fever broke out in Bermuda, which destroyed at least 663 persons, from July 1st to December 31st, out of a population of 13,860 persons—*i. e.*, in round numbers, 1 in 20. The mortality was greatest in St. George's parish and Hamilton. In St. George's and Hamilton parishes, the surface drainage only is attended to; there are no sewers; and the use of cesspools is universal. Hamilton has a population of 1900 persons, in 426 houses, four persons to each house, each house being provided with a tank to receive rain water. There are three or four wells in the town, the water from which is very bad, and used only by the lower orders of people. The cesspools are from fifteen to twenty feet deep, and never cleaned out. The shallow cesspits are cleaned out once a year, and then the contents buried in a hole or emptied on the beach. All dirty and waste water is generally thrown in the yard. The soil of Hamilton is of a light calcareous nature. The soil of St. George is of two distinct qualities, one part composed of a stiff reddish clay, with a limestone bottom; the other and greater portion, light and sandy, particularly porous, admitting immediate absorption of the heaviest fall of rain. In St. George's, there is no surface drainage or sewerage whatever; the population is 1440, in 440 houses—*i. e.*, six in each house. Cesspits are used, but the night soil is frequently deposited on the surface in all

directions. Water-butts are very uncommon, and many houses have no tanks. There are two or three wells, with very bad water. In this manner, a vast amount of night soil penetrates through the porous soil, or, floated to the sea, gives rise to the most offensive odour in the waters on that shore. For the very same reason, and owing to the proximity of the cesspools and the tanks, these are frequently mixed, and the water becomes very offensive. It was thus that the tank water at Fort Cunningham at last could not be drunk. As an evidence of the impregnation of the soil with fæcal matters, it was remarked that the submarine soil where the *Thames* hulk ship was placed was contaminated, so that the odour evolved during its violent commotion in a gale of wind was well nigh intolerable. In the *Minutes of Examination of the Sanitary Condition of Ireland* (No. 1, art. 3), it is stated: "The water in nearly all the tanks was found impregnated with sulphuretted hydrogen gas, and was only used by the inmates for culinary purposes, the water for drinking being procured from other sources." And again: "The tanks are also too close to the houses and to the privies, and thus receive impurities which affect the water." (p. 74.) This state of things (although especially applicable to Rows 3 and 4 of the mechanics' houses, being nine dwellings on the glacis) is yet sufficient to prove the character of the water partaken of, at least for culinary purposes, in which manner much poison could have been transferred to the alimentary canal. It is not everything used for alimentation, or every vessel cleaned which is previously submitted to a temperature of 212°, at which temperature ferments are destroyed. And it is a peculiar property of such localities, when once encountering an epidemic influence, to develope the poisonous ferment very rapidly, as before shown, in referring to Surgeon Jones's opinion. (See page 28). Hence one reason why, after a time, an infected locality becomes very dangerous, and all more or less pass under its influence. In the Boa Vista fever, although the natives suffered but little at first, afterwards they did suffer, and then more severely than the whites—probably because the whole waters drunk by this town were so impregnated with fæcal matters as to be too poisonous to be resisted. A very rainy season preceded this outbreak of fever.

I might have extended this paper in speaking of bad water in its effects in producing some other diseases; but,

as Dr. Richardson has so ably done this already, in his paper on the Supply of Water in London, I feel this is unnecessary now. I must however say, that it is possible the effects of the same poison in water or as an emanation may produce occasionally a different effect. But this is no proof of their non-identity. It is quite possible, where two allied diseases coexist, that the one may be produced by the ingestion of the same poison in water, the other by its inspiration as an emanation; and thus we may in the end be able to carry this distinction one step further. If we judge from the effects of medicine taken by the mouth, and inspired as a gas, the difference is marked, varying often not only in degree, but also in kind. Ether, chloroform, and alcohol, inspired, will often produce, in very small quantities (a few drops), effects as powerful as ounces taken by the mouth. The effects, however, vary in kind. Carbonic acid is a well known instance. The tincture of sesquichloride of iron inspired will produce faintness and vomiting, which it will not do taken by the mouth. The powerful and almost celestial feelings of inspired opium and cannabis Indica are frequently not even in degree producible by any dose taken by the mouth. In like manner, it is not impossible that the same animal poison, according as it is inspired or taken by the mouth, may produce very different results.

Such are some of the grounds which lead me to believe that fæcal matters may undergo in water changes such that, in their accidental ingestion by the mouth, they may prove injurious to the organism, and, either without further transformation or by additional changes taking place in the alimentary canal, prove the cause of disease. There is, however, this difficulty in tracing out these changes; it is not always possible to prove to demonstration that the emanations from morbid fæcal matters may not have likewise co-operated. Facts are often indistinctly recorded, where, with a little of Dr. Snow's perseverance and exactitude, precise ground for accurate conclusions might have been given. This is especially the case when biassed men are employed, or when the medical element is not sufficiently mixed in a board of commissioners, called upon to investigate the causes of epidemics. Much, however, I trust, has been stated to prove that fæcal matters in solution often produce fatal results, in cholera, dysentery, diarrhœa, and typhoid fever, and probably also in yellow and other bad intermittent fevers. If this be so, then the removal

of any sources of this infection, especially in the supply of water provided to communities, is imperatively called for, and should be enforced by the most stringent sanitary regulations.

PART III.

INJURIOUS INFLUENCE OF FÆCAL EMANATIONS.

In my first paper, I alluded to some of the contingent phenomena which accompany the evaporation or emanation of miasmata. I dwelt particularly on four of these: 1. A comparatively high temperature for the period of the year. 2. A light atmosphere. 3. Moisture in the air. 4. Ammonia, always present in the decomposition of animal and vegetable matters, and in contagious diseases.

The co-existence of the first two phenomena is also insisted upon by an able writer as co-operative in coal mine explosions. I allude to the very interesting paper by Mr. Dobson of Cambridge, on the influence of revolving storms in the escape of inflammable gas in coal mines. The propinquity of these storms is always marked by a *rise in the temperature and a fall in the barometer.* These storms have generally a south-west direction, and as they pass over a country, so, under the diminished atmospheric temperature, the inflammable gas in the mines escapes. In an opposite state of atmosphere, i. e., a fall in the temperature and a rise in the barometer, explosions seldom occur. And so it is with emanations productive of disease; only that moisture and ammonia are superadded. Indeed, I think it may be stated as a law in nature, that, whereas a moderately low temperature, dry weather, and a heavy state of the atmosphere, favour the deposition towards the ground or into water of poisonous miasmata, so a warm temperature, damp weather, and a light atmosphere, favour the ascent of these as emanations. But I believe more than this, that at certain low temperatures emanations are impossible. When I made this statement in my last paper, the truth of it was called in question; yet the statement is in accordance with fact and experiment; for then the fourth condition which I spoke of, is impossible, viz., the

evolution of ammonia. Neither vegetable or animal matters will decompose at a temperature below 32° Fahr., and even many degrees above that temperature. The ammonia which will not *rise* in vapour will be kept to the surface of the earth, even if found at all; and the same is true of watery vapour, though not to the same degree. A cubic inch of air, which at 0 Fahr. can only contain 0·856 grains of moisture, at 95° Fahr. contains 17·009 grains. The amount contained at 11° Fahr. is almost 0, while at 50° Fahr. 2.5 grains are contained. Hence the reason that in wet or damp weather the offensive vapours of a town are greatest. These odours, as I stated in my former paper, are, if smelt powerfully, conversely a sign of rain. What holds for vapour is equally true, therefore, of miasmatic emanations. This view explains readily why it is that in very cold weather, when the atmosphere is always most dense and heaviest, driest and most free from ammonia, we have precisely those circumstances present which are most unfavourable to the spread of disease arising from poisonous emanations, etc., which, experience proves, usually disappear at such periods. We may, therefore, I think, safely conclude that in proportion as the barometer is high or low, and the atmosphere dry or moist, so will miasmata remain on the ground or rise, and the diseases arising therefrom decrease or increase. But I think we may be justified in going further still, and reasoning from these antecedents, to lay down *à posteriori* this additional rule, that if a disease spreads in very cold weather, that weather being dry and the barometer high, the said disease cannot be generated by *miasmata* in solution with air, but is either contagious or propagated by poisoned water or other ingesta; and *vice versâ*, that if a disease spreads in damp hottish weather, with low barometer, it is most probably generated by *miasmata*.

So true is the concurrence of dampness in the generation of many epidemic diseases, that Dr. Barton, of the United States of America, has been led therefrom to form a theory on yellow fever, which is very ably supported by Dr. Hunt, of Buffalo, in the recent number of the *Transactions of the American Medical Association*. Without the conjunction of dampness, and what he calls *terrene* causes, no epidemic can occur. These *terrene* causes are any which may give rise to miasmata, such as upheaval of soil, decaying vegetable and animal matters, filth, low stagnant recesses, etc., etc. This theory has, moreover, the advantage of reconcil-

ing facts which were before in apparent contradiction, but which now appear to be in strict accordance one with another.

My friend Dr. Snow, I believe, while he does not deny that emanations may give rise to cholera, yet believes they have been raised in the air by watery vapour, in which water they had acquired their poisonous character, having been first voided in the evacuations of affected persons. This may or may not be true, but it at any rate directs attention to *watery vapour* as a chief agent in the transmission of the poison.

Together with this watery vapour, and ammonia, there are probably other gaseous compounds which, by their presence and quantity, influence or modify the character of the malarious poison in its action upon man. Unfortunately their precise nature has been of late but little studied specially, which is the more to be regretted, since organic chemistry has progressed so much. No doubt, like the perfumes in the atmosphere, of which we may become sensible from the sense of smell, and yet which we may be unable to detect by chemical agents, they are too subtle to be made out. Still there are particular symptoms which concomitant gases produce, which so far resemble these in their effects, that we are warranted, I think, in referring some of the results produced by contagious poisons, at least, to their concurrent action. This is especially true with regard to sulphuretted hydrogen; but carburetted hydrogen and also phosphuretted hydrogen may produce analogous effects. Of the two poisonous gases which occur in common sewers, described by Thénard, and before alluded to, the results of the first (oxygen, 14; nitrogen, 81; carbonic acid, 2; and sulphuretted hydrogen, 3) are not necessarily fatal, unless their inspiration is long continued; and the symptoms are those then of poisoning by sulphuretted hydrogen, being gradual prostration of the physical powers, giddiness, sickness, and general debility, finally emaciation and low fever; but the *post mortem* appearances are very similar to those observed in the worst forms of typhus. The second variety of gas found in common sewers (oxygen, 2; carbonic acid, 4; and nitrogen, 94; with more or less ammonia) produces its effects more suddenly, being of course quite irrespirable if it contain much carbonate of ammonia; or, if this be absent, killing by asphyxia, from the oxygen being in insufficient quantity.

A great deal is as yet unknown as to the effects of car-

buretted hydrogen and phosphuretted hydrogen when in a more or less diluted state in the atmosphere. That both should prove fatal if breathed singly can be readily understood, but in what manner they prove injurious when diluted with various proportions of atmospheric air, is not made out. Carburetted hydrogen breathed in small quantities has been recommended in cases of phthisis, catarrh, and hæmoptysis, though upon what theoretical grounds I cannot tell. As it is always present among the gases disengaged from swamps and stagnant waters, although in these cases mixed always with carbonic acid and nitrogen, it has been considered by some as the active agent in the production of fever from malaria. I believe this opinion is entirely without proof; but it is probable some of the effects of it might be inferred from the diseases peculiar to coal miners.

Of phosphuretted hydrogen still less is known. There are three compounds of phosphorus and hydrogen at least. The more inflammable, the phosphuretted hydrogen, which is emitted in the putrefaction of organic bodies containing phosphorus, which is the cause of the peculiar odour of decayed fish. Phosphorus we know to be a deadly poison; and if we were to reason from the analogy of arseniuretted hydrogen, we should be led to attribute very deleterious influences to any emanation containing it in admixture.

In connexion with the action of phosphuretted hydrogen, it is interesting to remark that a peculiar low fever is often said to be induced by the decomposition of fish and seaweed on the sea-shore in some watering places at certain seasons of the year, in which emanations the phosphuretted hydrogen exists. Liebig denies that this gas is ever generated in the decomposition of animal bodies, either in disease or in the putrefaction of dead bodies; and what is more important (he adds), "the human body contains no phosphorus in such a state as to yield by any process during life or after death phosphuretted hydrogen gas." (Liebig's *Letters*, Spont. Combust.) The presence of some phosphorised vapour is however undoubted, as it exists occasionally around parts in a state of ulceration, or rapid decomposition, as in dissecting rooms when accompanied with light, or in rare cases of phthisis with luminous breath. At any rate, from Dr. Ayre's experiments, we may infer that these gases are present in the atmosphere around putrefied fæcal matters.

According to M. Pariset, however, "The immediate products of decomposition are not simply elementary bodies, as hydrogen, carbon, and nitrogen, but, on the contrary, vapours, the organised condensable molecules of which are capable of being completely disorganised by a combination of heat and moisture, drawn along by gases, aqueous vapours, currents of air and dust, dispersed without being destroyed by pure water. Desgenettes believed that, expelled by sulphuric acid, they are only neutralised by the powerful alkaline bases, lime, potass, soda, ammonia, cinders of vegetables. They are condensed, on the contrary, and not decomposed by porous bodies, among which fine sand may be classed. While they preserve their peculiar organisation they can serve as ferments, and consequently act upon living beings to disorganise them, finding in them the degree of heat and humidity which ought to destroy them; they are not destroyed and do not undergo these metamorphoses but by making the being who has received them a partaker, whose life is thus compromised, sometimes destroyed. . . . When the elevation of temperature makes the ferments evaporate, man receives them by the superficial absorbent vessels, or by those of the lungs in the act of respiration." (*British and Foreign Medical Review*, vol. xxiv, p. 249.)

The precise amount of heat required for the development of this fermentation varies probably with each disease; it may, however, be generally stated, that a very high temperature destroys it, as well as a very cold. In yellow fever, Chisholm makes the temperature from above 60 to 90, below or above which it is inert.

There can be no doubt, however, that something more is required even in the due development of these diseases, and it is probably to be found in some *electric condition*. We know, for instance, that every man possesses electricity in his organism. The skin gives positive, the mucous membranes negative electricity. The atmosphere likewise possesses one kind of electricity, while the earth possesses the opposite. Man lives thus between these two in a kind of intermediate state, in which both electricities are in equilibrium, and thus escapes injury. This is especially true in dry weather. But there are at least three causes which may interrupt his immunity—all connected with several natural processes in action.

1. *Vegetation* developes positive electricity. Now, as before seen, this kind of electricity favours decomposition. Hence, if it be abundant, as in those instances where vege-

tation is rank and rapid, in a moist atmosphere, then disease may be rapidly developed.

2. *Evaporation* also developes in the same way positive electricity with the same deleterious influences; the earth remaining negative. Hence usually pestilential miasmata arise with evaporation; when this is greatest, disease is most rife, as when a hot season follows heavy rains. It may be opposed to this view, that positive electricity also augments with height, and yet mountainous regions are, as a rule, healthiest; but then, the temperature is also lowered, the air more rarified, and therefore containing less oxygen in a given volume, and lastly, more agitated by winds. Hence the miasmata, being weaker and not stationary, cease to be injurious.

3. Under certain circumstances, *clouds heavily charged with electricity* approach the earth. These, by disturbing unduly the equilibrium of man's organism, prove injurious. We may explain in this manner the peculiar sensations experienced by some persons in thunderous weather. If this disturbance persist long, and especially if the air contain much moisture, so that the electricity is easily communicated to persons, then the chemico-vital relations of the body may be seriously influenced, and disease result. Mists and damps are frequently associated with typhus; and in typhus, the normal electricities of the skin and mucous membranes are reversed. It may hereafter, perhaps, be shewn that the fermentative and diseased changes of mucous membranes are connected in this disease with the kind of electricity they have acquired. (See *Brit. and For. Medical Review.*)

We are now in a position to speak generally upon this part of our subject, reserving the individual instances where specific disease is generated for more particular inquiry when we speak in order of the individual diseases themselves. That town refuse and night soil give rise to the most fatal emanations is, I think, fully established by the researches of Mr. Grainger, Drs. Lewis and Sutherland; and yet, as Mr. Grainger informs us, many of even the educated classes believe now that they are innocuous and also curative of disease; innocuous, because many have been exposed to them, and yet have escaped disease; and curative, because the atmosphere of a cesspool is said to cure hooping-cough, and that of a cow stable consumption. So far may popular prejudice deceive even the most educated.

Here are a few instances taken from the *Reports on Cholera* of the Board of Health (p. 41). Immediately opposite Christ Church workhouse, Spitalfields, belonging to Whitechapel union, and only separated from it by a narrow lane, a few feet wide, there was in 1848 a manufactory of artificial manure, in which bullock's blood and night soil were desiccated by dry heat on a kiln, or sometimes by mere exposure of the compost to sun and air, causing a most powerful stench. The workhouse contained about four hundred paupers. Whenever the works were actively carried on, particularly when the wind blew in the direction of the house, there were produced numerous cases of fever of an intractable and typhoid form; a typhoid tendency to measles, small-pox, and other infantile diseases; and for some time a most intractable and unmanageable and fatal form of aphthæ. From this cause alone twelve children died in one quarter. In Dec. 1848, when cholera had already appeared in the union, sixty of the children were seized with violent diarrhœa in the early morning. The proprietor was compelled to close his establishment, and disease ceased among the inmates. Five months afterwards, the works were recommenced. In a day or two subsequently, the wind blowing from the manufactory, a most powerful stench pervaded the workhouse. The night following, forty boys were seized with severe diarrhœa, while the girls, whose dormitories faced in another direction, escaped; and the suppression of the nuisance resulted in the disappearance of the diarrhœa, and it has not since recurred. A similar effect was observed in St. George's, Southwark, in the summer of 1847, where a similar manufactory was established, and which also gave rise to most offensive effluvia. Diarrhœa, extensive and severe in type, soon appeared, which only entirely gave way when the nuisance was suppressed.

In the Potteries of Kensington there were kept, in 1849, 3,000 pigs, and as the process of fat boiling was also extensively carried on, an area of half a mile round was tainted by foul odours. The inhabitants there lived in a state of misery and filth which is indescribable. The houses were close to a pool of stagnant water, called the Ocean, which was covered with filthy slime, and bubbled with a poisonous gas. This Ocean was fed by the drainage of the pigstyes and privies. In this place, out of 1,000 inhabitants, in ten months of 1849, 50 died; 29 of fever, and 21 of cholera, *i. e.*, a mortality of 6 per cent.

per annum. Some twelve or thirteen hundred feet off, says Dr. Lewis, is situated a row of clean respectable houses, called Crafter Terrace, Latimer Road; the situation, though low, is clear and airy. On Saturday and Sunday, the 8th and 9th Sept. 1849, the inhabitants complained of an intolerable odour, the north-east wind blowing directly upon the terrace from the Potteries. Till this time, there had been no cholera among the inhabitants of the place. The next day, the disease broke out. Indeed, wherever there are fæcal emanations, it may be laid down as a rule that the place is unhealthy, and its effects debilitating to a degree. In such places, if a poisonous ferment be introduced, it will speedily develope and increase, and produce disease among those who are exposed to its action. Dr. Sutherland gives an instance in point in the case of the Witham suburb of the town of Hull, a space of some three acres, two acres of which are used as a deposit of the night soil and other manure. The town of Hull generally gives as the average age of death in other parishes, 23; in Witham it is 18.

All who visit among the poor must admit this; and here it may be as well to answer an objection to the argument made use of by Parent-Duchatelet, in regard to the healthiness of the inhabitants who live near Montfaucon in Paris. We are all aware that Montfaucon is the great receptacle of the fæcal evacuations of Paris, and dead horses, etc., which are all brought there in carts. Now it is alleged, that if fever could be generated from such a cause, the health of the workmen employed would be very bad, and they would be very obnoxious to fever, which is contrary to the fact. Indeed, if their state of health was to be taken as a criterion of its salubrity, this trade would be one of the healthiest for workmen. These *débardeurs*, it is stated by that author, are very rarely liable to intermittent fever; and those amongst them who have it were ancient soldiers of Africa, Spain, and Russia. It is not true that they are liable to cramps. Their colics are due to bad wine. The mania with which some are affected (those of Bercy) is due to white wine mixed with perry, drunk to the extent of six *litres* (about seven quarts) daily. Atonic ulcers are very rare among them; the only disease to which they are liable is *grenouille*, a peculiar affection of the extremities when placed in water. Typhus is very rare in this district as compared with others. The epidemic that prevailed in 1814 stopped on April 17th of the same year, and has not since recurred, though Montfaucon still exists.

Duchatelet published his work in 1836; and I have not seen any statistics to prove the comparative amount of typhus and low fevers in Montfaucon now as compared with other districts. Assuming that these statements are true, and even applicable to the present day, the exemption is to be otherwise explained. These *débardeurs* are, as compared with other Parisian workmen, *unusually well fed.* They have animal food—most Parisian workmen live exclusively on vegetables: and although this animal food is horseflesh, and therefore may be distasteful to an Englishman, it is not the less a wholesome food. Now wholesome food is as powerful a prophylactic against fever, as bad food and fatigue, and mental depression, are favourable to its development. I believe Dr. Alison looks upon such influences as directly causative of fever. Dr. Corrigan, in writing to me on this subject, says: "We have lost six or seven clinical clerks within the last three years, and in every case, I believe, from exposure while the system was in a depressed state. In one case, the student went into the wards after sitting up all night; in another, after great exhaustion from three or four hours skating, etc.; and so impressed are our old porters with this, that they constantly urge upon the young clerks not to go into the fever wards when relaxed, exhausted, or tired out; and I always warn them *against going in fasting.*"

Again, whatever effects the emanations have on the spot, it is notorious that puerperal fever is frequently generated in the lying-in wards of St. Louis's Hospital, in the Faubourg St. Antoine, when the wind blows from Montfaucon. This fact I have before mentioned, in my paper on Puerperal Fever. Whether these emanations acquire in their transit through the air the amount of oxygen and moisture necessary to ferment and putrefy, I cannot say, but the fact is undeniable. The spread of contagious diseases generally through fomites and the emanations arising therefrom, if soiled, is asserted on too many sides to admit of complete denial; and the liability of washerwomen to catch the diseases of those for whom they wash is almost universally admitted, and yet remarkable instances are given to the contrary. Washerwomen, says Dr. Corrigan, certainly do not suffer from their employment; nor do the nurses or ward maids employed in changing the linen, making the beds, etc. This contradiction of facts can be reconciled by two suppositions founded on fact, that either the parties exposed to the emanations therefrom are not fit recipients,

or in great measure to the chlorine employed, but particularly the strong alkali, the soda with which they wash, which destroys the poison effectively.

We will now notice some individual diseases which are capable of being produced by fæcal emanations.

Cholera. Pettenkofer is one of those, I believe, who states that diarrhœic fluids will, during choleraic seasons, develope through fermentation cholera poison; that this change depends much upon the nature of the soil—a damp soil favouring this fermentation, which a rocky soil in measure prevents; and that this cholera virus developes, according to the idiosyncrasy of the patient seized, cholera, cholerine, or simple diarrhœa. Whether this view be admitted or not, it cannot be denied that emanations from *cholera stools* will sometimes produce cholera. It has, I think, been fully proved by Drs. Alison and W. Budd, as is clearly shewn in the papers written by these gentlemen, and respectively published in the *Edinburgh Medical Journal*, Dec. 1855, and the Association Journal of 1854, that the poison of cholera is multiplied rapidly in the stools and fæcal matters generally, and acts chiefly in consequence of a particular decomposition which it subsequently undergoes in these, becoming therein so concentrated, that an emanation therefrom may produce in a recipient the disease. I think that one fact adduced is conclusive upon this point. Although the same water was drunk by all the inmates in a large workhouse, those only who made use of the same privies or receptacles for cholera dejections caught the disorder, while the recurrence of cholera seizures was effectually prevented by guarding the avenues leading to these, and by preventing persons not affected with cholera from using them. The miseries endured by our troops in the Crimea, who were obliged to encamp over the same filthy *locale* where the Russians affected with cholera had previously been quartered, is a sad confirmation of the same tale. Certain it is, that in Dr. Lindsay's hands, experiments made by him with the emanations from evacuations and blood, and from clothes saturated by the evacuations and breath of cholera patients, were followed by specific results. In his experiments, cholera was simultaneously developed in four animals, and proved fatal in two of these.

I am quite aware that instances at direct variance with these facts may be cited. Thus, in Mr. Marshall's paper before referred to, we read that Dr. Sarmichen, M. Foy, and

others, had purposely inhaled the breath of cholera patients with impunity. Drs. Deynert and Mavroyen did the same at Moscow. Persons have slept in wards as well as upon cholera beds, and have even worn the linen of cholera patients with impunity. Again, dogs have been made to inhale effluvia from choleraic evacuations and blood, and from clothes worn by and saturated by the perspiration and breath of these patients. Rabbits have been placed in close places with choleraic stools about them, but escaped. But these apparent contradictions are yet, I believe, to be reconciled. There cannot be a doubt, I think, from Mr. Glaisher's experiments, that the air of choleraic wards is sometimes changed. I have before alluded to Dr. R. D. Thomson's experiments in which he found that the air of a cholera ward was acid; also that the external air in choleraic seasons was alkaline only in the sewers. Whether this be a constant change present or not, is open to future inquiry. Still the disparity of action would point to a change in the chemical character of the surrounding medium; but if the concurrent circumstances, the intensity of the poison, the period of the epidemic, the fitness of the recipient, etc., and the varied contingencies to which I referred in my first paper will not account for this, I am sure the hygrometrical state of the atmosphere will. I again repeat that, if the atmosphere be very moist and warm, it will easily convey infectious particles; and if it be dry and cold, it will as effectually prevent diffusion of the cholera poison as a solid metallic or other not porous substance.

Cases, at any rate, are not wanting in which fœtid emanations in choleraic periods have given rise to cholera. I have already alluded to some; but I may mention a few more, selected from the *Reports of the Board of Health*, which I prefer making use of as being official.

I have already instanced the case of the Kensington Potteries; I have also mentioned Witham in Hull. Dr. Sutherland says that 91 deaths from cholera occurred here on the outskirts of a triangular space of little more than two hundred yards. Dr. Sutherland adds: "I have never known an open neighbourhood of this size yield so large a number of deaths." The report goes on to allude to Merthyr Tydvil, Dowlais, and Pen-y-daran, all in the same neighbourhood, where cholera was very rife and most destructive. The instance of Brest prison is also very instructive. This prison contains 2,662 inmates, distributed in four wards and in an infirmary. The four wards have twenty-seven water-

closets, so that the prisoners in each row may be enabled to reach the closet without being unchained. These water-closets communicate with a drain which opens into the harbour of Brest. At low water, the south-west winds (remark here the *damp* winds) blow up the unguarded drain, and force back the mephitic vapours into the very wards. The infirmary and condemned cells are free from this inconvenience: 189 cases, with 113 deaths, occurred in the prison; a mortality on the seizures of 59 per cent. Out of 2,445 prisoners in the wards, 165, or an average of 6·7 per cent. on the whole number, were attacked with cholera: while in the infirmary and condemned cell, out of 217 persons, only 3 were attacked, or 1·3 per cent. The same result occurred in 1832. Then 53 prisoners died in the wards, and one only in the infirmary. (p. 47.)

Of the eight deaths in Hampstead, four took place in localities swarming with night soil and manure; and those persons generally suffered most in Marylebone who lived over stables and cowhouses. (See *Board of Health Reports.*)

I might prolong this part of my paper almost indefinitely, so numerous are the instances recorded; enough, however, has been said to prove the proposition, and this will be sufficient.

Dysentery. The facts collected in regard to dysentery in some epidemics point to a similar origin for the contagion. Most of the older writers have stated the same thing, and believed that the stools were contagious. "You often see," says Hildoanus Podoanus, "the disease attacking those who use a clyster-pipe previously employed by a dysenteric patient, and not well cleansed." Around a night-chair or a water-closet, the source of contagion may persist for a time, and infect all those who employ them. Fabricius Hildanus, in his work *De Dysenterica*, also believes in this infection from stools. (*British and Foreign Medical Review*, vol. xxiv, p. 357.)

This same fact is recorded by Zimmermann (*Ib.*, p. 139) as follows. After the battle of Dettingen, the dysentery made its appearance in the English army, and committed great ravages in it during the months of July and August. The hospital was in the village of Feckenheim, about a league from the camp. During the time that the army lay near Hanau, about 1,500 sick, besides those wounded on the field of battle, were brought from the camp into this hospital, and amongst these the greatest part were ill of the dysentery; the air was by this means infected to such a de-

gree, that not only the rest of the patients, but even the apothecary, nurses, and the other servants, with most of the inhabitants of the village, were infected. (Zimmermann *on Dysentery*, p. 137.) Pringle says that this epidemic arose apparently from sudden changes of temperature and great exposure of the troops, and afterwards became contagious from development of typhus or hospital fever, but that, when the troops left the village, which had been converted into a hospital, and which was filled with the dirty straw covered with the dysenteric excretions, the disease ceased immediately to be propagated by contagion.

"Now," says Zimmermann, "the contagious power of the dysentery lies chiefly in the excrements; for the mere smell of them has often communicated the dysentery to men in perfect health and even beasts." And further on, he adds: "And even though one stops one's nose, one is not secure from infection, for the putrid vapours adhere to one's clothes, and when they are in high degree contagious, are thus communicated from one person to another; while at the same time he that has the clothes on his back is perhaps not in the least infected." (Zimmermann *on Dysentery*, p. 19.)

Himertus also admits this source of contagion. Vignes remarks: "We have seen many sick contract the disease in going to the common places where the dysenterics render their stool. (p. 205.)" And Chomel holds the same opinion. (*British and Foreign Medical Review*, vol. xxiv, p. 357.)

The case mentioned by Lind in his first paper on Infection, where a patient with chronic flux is said to have infected all those who used the same necessary with him, is believed by the author of the review above quoted, to be apocryphal; but to my mind it is quite within the pale of belief. (*Ibid.*)

The following is another instance of the special effects of stools in continuing the disease.

A very severe epidemic of dysentery broke out at Morrovelho, among the miners of a large gold mine in that locality. All the patients admitted into the hospital for other complaints, wounds, catarrhs, etc., were seized soon after their admission with it, and many died. My informant (Mr. T. P. Champion), a visitor in the hospital, always walked through the wards every day, and, observing that the stench from the patients' evacuations left in open pans under their beds was very great, he mentioned it to Captain Cotesworth of the Royal Navy, at that time chief-

commissioner of the Catabranca mine, another mine in the neighbourhood. This gentleman then stated to him that no doubt the disease was aggravated by this custom. He instanced as proof the case of H.M.S. *Dublin*, in which the same complaint being rife in the ship, the men attacked were, because of the supposed contagion of the stools, kept apart and quite separate from the other men, and also prevented from going to the same closet. My informant mentioned this fact to Mr. Crickett, the chief commissioner at Morro-velho. After speaking to the surgeon, orders were given to remove these pans, to purify the closets, and to keep the wards sweet. The good effects were soon apparent in the marked diminution of the cases of dysentery, and finally in the speedy disappearance of the disease.

Dr. James Bird has informed me that the same fact was observed in the cases in hospital during the first Punjaub expedition; the emanations from the stools reproducing the dysentery in other cases, but, what is more singular, developing the hospital gangrene among the patients labouring under wounds to a fearful degree. During this period, the air was hot and moist. The mortality from disease in this expedition was very large, as compared with that of the second Punjaub expedition, during which period the weather was cold and dry, and the health of the troops good. This difference proves the danger and the influence of moist heat in spreading disease.

Typhus and Typhoid Fever. In the general remarks which preceded this paper, I already instanced the case of Christchurch Workhouse, Spitalfields, where typhus and typhoid fever were generated from this cause. I may quote here Mr. Grainger's evidence on this point. "A large body of evidence which I have received from medical practitioners both in London and other populous towns, as Manchester, Liverpool, and Nottingham, distinctly proves that persons habitually exposed to such an atmosphere [that of privies and cesspools] are thereby predisposed in an especial degree to fever and other sickness; and that, in courts and alleys, those persons who reside in the houses immediately adjoining foul privies, all other circumstances as to food, lodging, etc., being equal, suffer more from typhus than the other inhabitants. So much is this the case, that houses so situated have been pointed out to me by the medical officer as being the constant seat of fever, families after families coming to reside in them, and all in succession being attacked." (*Report on Cholera*, p. 87.)

My own experience among the poor proves the same fact; added to which, in many instances, with the removal of the nuisance, so the disease has disappeared. An interesting and forcible instance was related by Dr. Thudichum, at a former meeting of this Society. Munich is remarkable for typhus; indeed, it is almost impossible for a family to live in it without some members catching the disease. Pettenkofer offers the following explanation. Two-thirds of the town possess no drainage, and the fæcal accumulations of the inmates, gathered in pails, are emptied occasionally in large holes in the adjoining yards, and left to accumulate and putrefy therein. Hence not only is the air polluted, but the ground in many places is saturated with liquid fæcal matter, whenever sufficiently porous to allow of its transudation. In these localities, therefore, typhoid disease is very rife; and, during cholera epidemics, the cholera has proved very fatal. This example is open, however, to the objection, that the disease may, from the very saturation of the ground, have been also produced by the impurity of the water drunk there when contaminated with choleraic matters in the wells; and that, as such, it is not a perfectly pure instance of typhus communicated by fæcal emanations.

Still, researches are not wanting to prove that emanations from cesspools have produced typhus. The cause of the disease is thus stated in Dr. Arnott and Kay's Report:—"The disease was most severe in those courts and alleys where there was no circulation of air; such, for instance, as Johnson's Change, in Rosemary Lane, in which there are about twenty houses, in almost every one of which fever prevailed. The disease first made its appearance there in the month of August last, and, on my first visit, I found the intolerable nuisance of the overflowing of a cesspool or privy, which continued for some time, there being no sewer to carry off the soil." (Arnott and Kay's *Report of the Sanitary State of the Labouring Classes.)* A case of typhus, produced from this cause, was mentioned to me by a medical friend. Being called upon to visit a patient affected with pleurisy, he was very much affected on entering the room by a most offensive cesspool odour, which entered the room by an open window, from a back yard in which the cesspool was exposed. The patient was removed to the hospital; but the same evening my friend became seriously affected with typhus, with black tongue, shivering, etc., etc., under which disease he was long ill. The

preceding good health, the instant nausea and uneasiness produced on entering the patient's room, and the rapid supervention in six hours or so of typhus, leave no doubt but that the cesspool odour was the exciting cause.

But upon this point it is needless to enlarge. Foul drains are notorious as at least being very frequently concurrent causes of typhus; but their effect in rendering suddenly malignant many of the exanthemata and other diseases, as scarlatina, erysipelas, measles, childbed fever, etc., when those labouring under these diseases are exposed to them, is so universally acknowledged, that any further remarks to prove this influence by a narration of facts becomes unnecessary.

Plague. That emanations from putrid animal matter will produce plague, is shown by the following extracts from Pariset. "In the winter 1823-4, the Pasha built a cotton factory at Kelioub, a small town four leagues north of Cairo. The foundations of this factory were laid among ancient and recent tombs. One day, about noon, a mason complained of headache. He was sent home, and at four o'clock he was dead. He was not visited, but eight persons who composed his family died the same evening, with both buboes and carbuncles. Kelioub was soon affected, and, of 5,000 inhabitants, lost 2,000. . . . Lately, a Copt rebuilt his house, and, on arriving at the burial-place, the workmen, to the number of four, took the plague." (Pariset, p. 938-9; *British and Foreign Medical Review*, vol. xxiv, p. 249.) That fæcal matter may retain the plague poison, is but a natural conclusion, and one much strengthened by a fact adduced by Mercurialis. Particular facts, however, in direct illustration of the position, are wanting.

Yellow Fever is the last division to which I shall refer in this paper. In the report of the Bermuda fever of 1850, we find the following statement, from the evidence of Captain R. W. Lacy. It is very analogous to the origin of cholera, before spoken of, at Brest. After speaking of the remarkable humidity of Bermuda, and in relation to the casemate barracks at Ireland Island, he adds: "On the north-west, and adjacent to the barracks, is a privy for the accommodation of the non-commissioned officers and men, the ineffective drainage of which, arising from its faulty construction, of its being beyond the influence of the tides, cause the soil, after ascending a large shaft, to accumulate at the foot of walls of the fortifications, emitting gaseous exhalations of the most pernicious and offensive nature, which taint the

atmosphere of the barrack. To the north-east is a building of a similar description, for the officers, almost equally objectionable." (p. 90.) Upon this point, Mr. Anderson states: "Regarding the locality occupied by the troops at Ireland Island, I consider that the barrack privy, which accommodates a garrison of about three hundred men, must exercise a bad influence upon its sanitary condition. A hollow shaft, about fifty feet high, and averaging about twenty-four feet by four, or ninety-six superficial feet of section, conducts the soil from the top of the rampart to an open ditch leading to the sea. The shaft is always filled with the most noisome gases, continually escaping; and the ditch with the decomposing soil. The whole surrounding atmosphere is affected, and that not only as regards the barracks, but as regards part of the naval stores." (p. 77.) The same fact is alluded to in the Minutes of the Sanitary Commission, consisting of the naval storekeeper, deputy inspector of hospitals, and clerk of the works. (p. 74.) In confirmation of this point, it may be said, that the mortality was 36·8 out of 505 cases in Ireland Island, as a ratio to 1000 of strength. The measure of this mortality is not, however, so great as that which would refer much more to the influence of bad water suspended; since, in St. George's and Hamilton, the proportion to 1000 strength was respectively, 831 cases to 184·2 deaths, and 436 cases to 39·4 deaths.

PART IV.

RULES OF TREATMENT TO BE ADOPTED.

If the views which I have enunciated are correct, and the development of the diseases mentioned, whether communicated by fæcal matter in substance, in solution in water, or through a moist atmosphere, is due to a process of fermentation which generates the poison, it is clear that the remedies to be adopted are those which arrest fermentation, or, at any rate, those which, if they do not exactly arrest it, either absorb or modify the character of the poisonous sporules or gases formed during the process of fermentation, and so impede their action. We have, moreover, to consider the treatment in two points of view.

First, in its *curative* effect, as relating to an individual already affected.

Secondly, in its *prophylatic* effect, as relating to communities, to prevent their being affected. Both points of view have the closest relation, although they are best separately considered.

1. *Curative Effect of Treatment.* The remedies here applicable are those called antiseptics; and they consist of all those substances which chemically act on the ferments, because they have an affinity for them, and thus bring about an equilibrium in the fermenting body. "Among these we may enumerate alkalies, mineral acids, concentrated vegetable acids, volatile oils, alcohol, sea salt (in excess). The most effective is sulphurous acid, the metallic salts, especially those of mercury. Arsenious acid does not prevent the putrefaction of blood, nor the ordinary alcoholic fermentation of sugar, but entirely suppresses the putrefaction of the skin and gelatinous tissues." (Liebig's *Letters on Chemistry.)* Chlorine retards putrefaction, as before stated, by forming with dead animal matter a white pearly compound, which is almost imputrescible; phosphorus and deutoxide of hydrogen, by absorbing oxygen; creasote and ammonia also retard putrefaction, as before shewn. We may also get a practical lesson from the processes employed for the disinfection of manures, which at least prevent the generation of emanations from them, if they do not completely arrest fermentation. "The disinfection of manure has for its object to destroy the offensive odour, and to cause the phosphates to be retained in a form capable of being assimilated by the plants" (Liebig and Kopp's *Report,* p. 439, 1850); and a great variety of processes have been adopted. One of these has for its object the formation of charcoal, which may be procured largely in several ways. The slime and mud of the Seine in Paris, as also of the Thames, contains very much animal and vegetable matter intermixed with it, which, if burnt, constitutes a very efficient kind of charcoal for this purpose. This is Mr. Salmon's method. Mr. Alfred Samson found that the cinders of peat or turf, or the simple refuse of carbonised peat, had the same effect. The bran obtained from sawing wood, the refuse of oak which has been used for tanning leather, the mould on the Parisian strata, answer the same purpose; and, when fæcal matters are mixed with simple clay, it is only necessary to carbonise this admixture, and we obtain at once a perfect disinfecting powder. Hydrochloric acid

is also said to act as a powerful disinfectant, by fixing the ammonia. The tribasic phosphate of soda has also been praised as a disinfectant. Calrond, in his *Journal de Pharmacie*, quoted by Liebig and Kopp (*Op. cit.* and *Journal de Pharmacie*, xvii, p. 281), finds that the methods usually employed do not fully satisfy any of the conditions required, but that the mother liquor of salt-works can be used with great advantage, and most perfectly, for disinfection.

It is impossible to look over this list of antiseptics and disinfectants without being struck with the fact, that among them are to be found most of those remedies which we have been in the habit of hearing so vaunted in practice for the cure of the diseases spoken of in this paper. I will instance a few of these: *sulphuric acid*, a mineral acid, found to be most useful in cholera; also *cajuput*, an essential oil, in the first epidemic in this country; *chalk*, an alkaline earth, in the premonitory and other diarrhœas; *saline injections*, *common salt*, and its internal administration in *large* quantities, as in the case of the late Mr. Carmichael of Dublin; *mercury*, according to Dr. Ayre's plan; and lastly, *charcoal*, which proved so invaluable a remedy among the choleraic patients in Canada in 1832. The disease was then at its height, when a stranger made his appearance in the colony, and administered charcoal largely; and the result was, that cures were very frequent, and the epidemic soon disappeared. The effect was so marked, that this stranger was believed by the ignorant to be a saint, having succeeded where the doctors failed. I have also been told that phosphorus proved in many cases, in the hands of a London practitioner, a most efficacious remedy. Dysentery is another disease in which antiseptic remedies have been given with success, such as sulphate of copper and alum, metallic salts; not to speak of small doses of calomel, another metallic salt, opium and carbonate of soda: the last an alkali. Charcoal has also been found most efficacious in dysenteries and diarrhœas, especially when complicated with intermittent fever. It is known in Smyrna to exert sometimes a curative effect in intermittent fevers, where quinine has failed; and this fact is confirmed by the experience of the learned Dr. Calvert, Calsagno, Macadino, Morsia of Nissoria, Burza of Palermo, Borland, and Tully. (See *Edinburgh Medical and Surgical Journal*, vol. x.) Perhaps, indeed, bark itself acts as an antiseptic. From some experiments made by Dr. M'Bride, it would appear that, if putrefaction has not proceeded to a very great

degree, it may be arrested by infusion of bark. Thus, Dr. M'Bride placed a putrid rat in a strong infusion of bark: it was completely deodorised thereby. If the putridity, however, was far advanced, the infusion failed. Certainly the change effected in many low fevers by bark is well explained on this view. Pariset believes that the reason that the plague does not produce a greater mortality in Egypt is, that the Nile water is very alkaline, and that spreading, in the annual inundations, over the country, it neutralises the miasmata. Most of the remedies I have mentioned have been employed in fever, and each would no doubt find some advocate in this Society. Charcoal is less often given in fever in this country than it might be. I am satisfied that, in my hands, it has been a means, under Providence, of doing much good in cases of fever, particularly when these have been accompanied with very offensive stools, especially in the remittent fever of infants.

It would be useless in me, however, to extend on this point. If the diseases in question be induced by fermentation, and especially in those cases where that fermentation is of fæcal origin, the remedies which common sense would indicate are those which arrest this action, namely, antiseptics and disinfectants. This, it appears to me, is the only philosophical treatment, and so far it is that which theory and practice alike support. Till, however, we know more of the peculiar chemical nature of each poison, we must, when once the broad principle is laid down, prosecute by empirical experiment. It may be, and it no doubt is so, that there is a specific antiseptic for every disease of the kind specified, and for many more. Sulphuric acid may be the best in cholera; mercury or wine in fever, as the case may be: furthermore, each may be susceptible of a similar explanation to that given for arsenic, which may be supposed to prove sometimes the best remedy for skin-disease, because it specifically arrests fermentation in the skin. But, at any rate, the remedies to be sought for are, theoretically, to be found among those which, by arresting fermentation in the system, prevent its extension.

While I state this, however, I must guard myself from an inference which may be made. I do not say that all fevers, or even the diseases here mentioned as fermentative, may not be cured by remedies which act otherwise than as antiseptics. This is quite possible, and I am bound to ad-

mit it. I merely suggest one mode in which many remedies may act.

2. *Prophylactic Effect of Treatment.* The treatment must be *prophylactic*, as well as *curative*. In a sanitary point of view, the remedies used must not only aim to cure the local malady in the patient's body, but to prevent its diffusion through a town, or wherever a number of individuals are congregated together. Now, as means to this end, our attention has of late been directed to two agents, both of which have been recommended—*charcoal* and *fresh sea water*.

(a) Charcoal. I have before spoken of charcoal as an antiseptic, although, from Dr. Stenhouse's inquiries, it appears to be a deodoriser from an opposite property, by reason of the large quantity of oxygen contained in its pores, which it absorbs, together with the miasmata and bad smells of decaying substances. These last are rapidly burnt and destroyed; so that, in point of fact, charcoal is not purely an antiseptic, but a *destructive* of decaying matters. A series of very valuable papers have been contributed, on this subject, to the *Journal of Public Health*, which have fully brought out the advantages of this agent as a disinfectant and purifier. Dr. Stenhouse's plan of air-filters, applied over offensive gulley-holes, would go far towards removing the foul odours of our London streets; and a similar kind of apparatus, to cover the apertures of water-closets in private houses, but especially in hospitals, would very much increase the comforts of their residents. In like manner, if bodies were buried in charcoal, according to Dr. Richardson's suggestion, all odour from this source would be removed, and not conveyed by solution, after heavy rains, within the common sewers. But this will not suffice. The sewers themselves will continue to emit these odours. All gulley-holes cannot be closed by charcoal air-filters, and the mud and rain must pass downwards into them through some openings; and it is therefore necessary, if possible, that something should be devised to deodorise the common sewers themselves. The employment of hydrochloric acid, chloride of lime, and pure charcoal, would involve too much expense—the first two in particular. In a very able paper by Mr. Durden in the *Journal of Public Health* (vol. i, p. 213), on the Preparation of Charcoal, the preferable mode, because the cheapest, would seem to be by the action of sulphuric acid on sawdust. The chief objection, however, to this plan, is the great uncertainty of

procuring the *raw material*, the sawdust, in sufficient quantity—a difficulty on which many a city company, whose profits, as set forth in their prospectuses, were to have been extraordinarily great, has foundered. But I think that we need only follow out in London Mr. Salmon's plan, already adopted so successfully in Paris. Assuredly, old Father Thames contains animal and vegetable matter in greater abundance than the Seine in its slime and mud; and since, when this is burnt, it yields a charcoal in a very fine state of powder, and which possesses the disinfecting property to an extraordinary degree, we have at our doors a source whence charcoal can be obtained in abundance and of excellent quality, and whence it could be very cheaply collected and prepared.

If once obtained in sufficient quantity, nothing could be easier than its transmission, by suspension in water, into the common sewers, from certain high localities before selected and established. We should do what we are now doing when we flush the sewers, only we should deodorise at the same time, instead of forcing upwards through the pipes of water-closets, and all the gulley-holes in town, those pestilential odours which are so fearfully obnoxious, and which originate or aggravate disease.

I am not aware that any but one objection of weight has been made to this introduction of charcoal, and it is that in course of time the sewers would become clogged up, and in this manner the flow through them arrested. I believe that this supposed effect is greatly exaggerated. The fine mud and broken up fæcal matters have not usually this effect. And if the supply of water passed through the drains at the same time be great, so that the current in the sewers is increased, and the size of the sewers be sufficient, I think no such effect would follow. Indeed, if obstruction did occur, perhaps in the end it would prove of advantage, because it would compel builders and the parochial authorities to repair and widen the narrow and faulty sewers, and to build any new sewers required upon an improved and more scientific plan.

(b) Water. I now come to the next point, the supply of water required for the suspension and diffusion of this deodorising powder. Under this plan, impure water would offer no impediment. The most offensive and impure water loses all odour by admixture with charcoal. All that would be needed would be a large reservoir with a smooth bottom, from which the water, mixed gradually with charcoal and

agitated by machinery (such, for instance, as a paddle wheel) could be emptied in quantity and at fixed intervals into the sewers. The foulest Thames water, or the Serpentine water of the parks, would suffice. I think this supply would be found sufficient; so that there would be no need to bring sea water into London according to Mr. Fuller's plan, which I believe is upon the whole very objectionable, at least so far as relates to the watering of the streets by it. From the facts before recorded on the subject of sea water, putrefaction and foul odours would be augmented, at least in the hot weather. I may add a few additional facts on this point. In a high temperature, at least so far as my experiments go (I speak of a temperature above 60°), animal matters will putrefy more readily and quickly in sea water than in fresh water. I have often noticed this result in vivaria. If a snail or a fish died in a *fresh water* vivarium, it might remain in the basin three or four days and longer, and the water did not sensibly suffer; the remaining animals escaped with impunity. In the *marine* vivarium, if a periwinkle or anemone died, in two or three days the water smelt strongly, and the remaining animals all died. This result I have frequently noticed in the same conservatory, where both vivaria were exposed to the same influences. I have also noticed a fact which, so far as *chloride of sodium* is concerned, seems to prove that a small quantity of this salt rather favours decomposition. I was preparing some of Liebig's beef tea: the temperature was tepid, and it was apparently fit to drink. In this state I thought to make it more palatable by the addition of a small quantity of salt. I added this, and the result was that it suddenly frothed up, and became very offensive; putrefaction had begun. Matters to be kept must be highly salted, and the temperature must be low, otherwise they will putrefy as readily, if not more so, than if not salted. In the instance of the Bermuda fever, to which I alluded before in my last paper, I instanced the case of the mud in which the convict hulk the "Thames" was fixed; although the water over it during the tidal rise was changed twice daily, the odour was very powerful from decomposing animal, especially fæcal, and vegetable matters. Sea weed will readily putrefy under a hot sun, though saturated with sea water. I therefore cannot help thinking that the beneficial effects of sea water have been overrated. Still I think that, if undiluted with much water, it does not in itself favour the extension of some epidemic influences. As a rule,

watering places, and particularly those where there is no fresh water river at hand, are healthiest; and except where cases are continually imported, or where bad water is drunk, epidemics, such as cholera, for instance, are not propagated. The coexistence, however, of a river, by the admixture of the waters of which the sea is unduly diluted, more especially if offensive sewage forms any part of this admixture, has quite an opposite effect. In such localities, some epidemics when present rapidly extend, and are very fatal. I see, then, no good result likely to be obtained by the substitution of *sea* for *fresh* water for the purpose of watering the streets.

I have said that the most impure fresh water would become purified and deodorised by the charcoal admixture. But the adoption of this plan should not be limited to the common sewers. There are *two* sources of infection *without* the sewers to which the deodorising measure should be applied. 1. The ordinary water-closets in domiciles should from time to time have charcoal thrown down into them; and whatever is inconsistent with its transmission, such as syphons or old fashioned traps, removed. If this were done, the source of annoyance in our houses from these odours would be removed. 2. We all know to our cost how offensive are mews, particularly in summer; and as before stated, during the late epidemic of cholera, it was the inhabitants of these localities who suffered most. If, in these, alternate layers of charcoal and stable refuse were interposed, the odour would in great measure disappear. I am aware, however, that John Bull could only be compelled to do this by an act of Parliament.

In any general plan, however, which should be adopted, it is expedient that the sewage matter now deodorised should be diverted from the Thames and carried out of town. But I think it is one of the advantages accruing from the employment of charcoal, that, once deodorised, there is no need of conveying it to such great distances from London. A distant removal has its advantages as affording larger space for operations, but then the tunnelling of the main sewers to such a locality is peculiarly expensive. If the deodorising process be effected in the sewers, a Montfaucon in London, even as near as that in Paris, would cease to be so great a nuisance; and the air being no longer tainted by vile odours, and not capable of being wafted back on the town, all disadvantages of proximity would cease. How near this proximity should be is

then, after all, resolved into a matter of convenience to the public.

To *agriculturists*, the advantage of such a ready made manure at the very doors of every large town would be infinite. Human manure is inferior to none, and equal to guano, and could be procured at a price considerably less. To the *inhabitants of towns*, a comparatively pure atmosphere, a less frequent recurrence of pestilence, and a more benignant type of disease would, under God's blessing, be the well earned reward.

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